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ORIGINAL ARTICLES.

I.—Note on a Collection of Carboniferous Trilobites from the Banks of the Hodder, near Stonyhurst, Lancashire.

By Henry Woodward, LL.D., F.R.S., Pres.G.S.

(PLATE XIV.)

EARLY in 1893, the Rev. G. C. H. Pollen, S.J., of Stonyhurst College, was so kind as to send up to me for examination a collection of Carboniferous fossils, obtained by himself with the assistance of Mr. William van Waterschoot van der Gracht, Jur. Stud., who during his residence at Stonyhurst paid much attention to the geology of the neighbourhood.

The collection proved to be an extremely interesting one, and Mr. Pollen most kindly requested me to keep a selection of the specimens for the Museum, only returning him a named duplicate collection for Stonyhurst. Subsequently Mr. van der Gracht sent me all the specimens he had himself collected, so that I was able to study a very fair series of the fossils from these beds.

The specimens sent up were carefully examined by Mr. R. B. Newton, F.G.S., and were found to comprise as follows:—Fragments of Vertebrata (possibly phalangeal bones of a small reptile?), also a part of jaw of a small Fish? (not determined).

	Recorded horizon.
Mollusca—Cephalopoda—	
Discites (*Nautilus*) *sulcatus*, J. de C. Sby. .	(Carboniferous L.).
Orthoceras læve ? Fleming	(,, ,,).
,, *cylindraceum* ? Fleming . . .	(,, ,,).
,, *pyramidale* ? Fleming	(Yoredale series).
Lamellibranchiata—*Entolium* (*Pecten*), sp. nov. . .	(? ,, ,,).
Brachiopoda—*Athyris lamellosa* ? Leveille	(,, ,,).
Chonetes, sp. ?	(? ,, ,,).
Productus, sp. . . . . (spines of)	(? ,, ,,).
Polyzoa ?—"*Palæocoryne*" (very doubtful)	(Carb. L. shales).
Trilobita—*Phillipsia van-der-Grachtii*, H. Woodw. .	(,, ,,).
,, *Polleni*, H. Woodw.	(,, ,,).
Echinodermata—Numerous ossiculæ of arms of Crinoids	(,, ,,).

My first impression, and that also I find which was entertained by Mr. Pollen, was that we were dealing with beds of the age of the Yoredale series, and this view was confirmed by a reference to the Geological Survey map, upon which the beds along this part of the Hodder are so coloured.

In the Centenary Record of Stonyhurst College, published in 1894, by the Rev. John Gerard, S.J., I am quoted, at p. 278, as having stated that the Trilobites obtained by Mr. Pollen from the banks of the Hodder "represent *probably* the *highest zone* in the

Carboniferous series from which this ancient group of Crustacea has yet been obtained." At that time, also, I had *provisionally referred* these specimens to *Phillipsia Eichwaldi*, Fischer, sp., and to *Ph. Colei*, M'Coy; but in the question of the geological horizon at which they occur I had not then enjoyed the advantage of Mr. Tiddeman's opinion, nor had I visited the spot and examined the beds *in situ*. I should therefore wish to modify the remarks I then made, both as regards the species of the Trilobites, which I now consider to be distinct, and also as to the exact bed they occur in, which, in deference to Mr. Tiddeman's judgment, I am led to consider must be placed lower down in the series.

As I was very desirous to ascertain what the geological horizon of these Trilobites really was, I decided to pay a visit to the locality, and, being most cordially invited by the Rector, the Rev. Herman Walmesley, S.J., and by Mr. Pollen, I started for Stonyhurst on the 10th of September, where my friend Mr. R. H. Tiddeman, M.A., F.G.S., of the Geological Survey, most kindly met me, and spent a day and a half going over the ground most carefully with Mr. Pollen, Mr. P. Stapleton, and myself.

Mr. Tiddeman pointed out to me that, in his opinion, we were dealing with the shales and limestones overlying the Clitheroe Limestones, and that our Trilobites came from near the top of this series.

TABLE OF THE CARBONIFEROUS ROCKS IN CRAVEN.[1]

Southern or Bowland Type.	Feet.		Feet.	Northern or Yoredale Type.
Coal-measures (Ingleton)	1500		...	Coal-measures (Incomplete near the faults in this area.)
Millstone Grits	3900		...	Millstone Grits (Incomplete near the faults in this area.)
Bowland Shales	300—1000		400—900	Yoredale Series
Pendleside [2] Grits (inconstant)	0—250			
Pendleside [3] Limestone (with Knoll-Reefs)	0—400	The great Craven faults.		
Shales, with Limestones (Trilobites occur here on R. Hodder)	2500		400—800	The Carboniferous Limestone (with conglomerates at base)
Clitheroe Limestones (with Knoll-Reefs)	×3250			
	No base.			

"These beds," said Mr. Tiddeman, "were marked as 'Yoredale Beds' in the earlier edition of the Survey Map, but after the

[1] Proceedings Yorkshire Geological and Polytechnic Society, 1890, vol. xi. pt. 3.
[2] [3] Both these sets of beds are considered to be absent on the Hodder.

intervening country between this and Wensleydale (=Yoredale) had been mapped in detail, the name was removed from the index in the later edition of 1891.

"It was not found practicable where, as in this case, the inconstant Pendleside Limestone and Pendleside Grits (see Table *supra*) were absent, to draw any definite boundary between the 'Bowland Shales' above and the 'Shales-with-Limestones' below, and both were coloured the light-blue shale colour."

In his paper referred to Mr. Tiddeman writes:—"Until the area of which we now speak was carefully surveyed it was assumed that there was a rapid transition of type in the Carboniferous rocks between Clitheroe and the big fells north of Settle and Malham; but as to the cause of such a rapid change no explanation was forthcoming. Even Prof. Phillips, who knew the country perhaps best of any among the old pioneers of Geology, often expressed himself to me as quite unable to account for it. If there is one thing more clearly brought out than another by the mapping in detail of this ground, it is this, that there is absolutely no transition from one type to the other. The two types run unchanged in their respective areas and with complete discordance with each other, quite up to a common boundary where the differences are rather accentuated than smoothed down. They might be Jews and Samaritans, agreeing in nothing save a common boundary to their territories and a determination to have nothing to do with one another.

"The line of demarcation is given by the Craven faults, and more particularly by that which runs by the south end of Malham Tarn, and that which passes between Malham Cove and Malham."

"The rocks on both sides of these faults seem to have been formed on slowly subsiding areas, but the Bowland area appears to have been subsiding more quickly and to a greater extent than the area occupied by the Yoredale type. It represents the downthrow side of the faults. The other side of the faults, of course, is relatively an upthrow" (see Table *supra*, York. Geol. Soc. Proc. 1890, vol. xi. pt. 3).

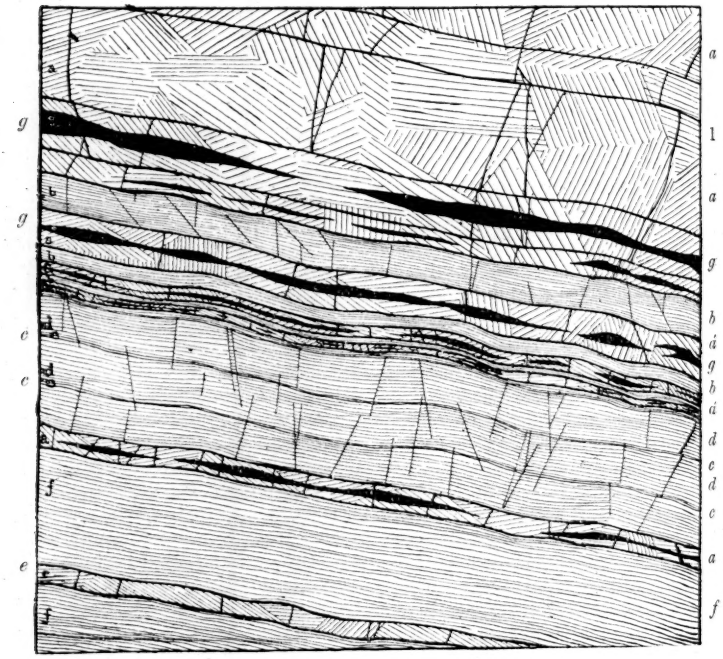
The subjoined brief note of the locality was drawn up for me by Mr. Pollen a year ago, and I now reproduce it in his own words, with the section of the beds which he also caused to be made for me with the kind assistance of Mr. van der Gracht.

Mr. Pollen writes:—"The River Hodder makes a straight run of about half a mile, in a direction E. by S., having Hodder School ('Hodder Place,' on the Ordnance Maps) on the right bank. Beneath this house the bank is very steep and high, but there is a broad shelf at the bottom on which are built the bathing-cots of Stonyhurst. The rock all along here is limestone, very crystalline and with much chert, alternating with clay-shales. The total thickness of the 'Yoredale series'[1] here is certainly over 500 feet, but the

[1] I retain the term "Yoredale series" here in copying Mr. Pollen's note, although we may follow Mr. Tiddeman in considering them to be lower down in the series than was supposed twenty years ago, when the country was originally mapped by the Geological Survey.

strata are so contorted that it is difficult, or impossible, to measure them accurately. Above are the grits and sandstones of Longridge Fell. The dip of the strata is here 20° W., but hardly 100 yards of the river-bank can be found without irregularities, faults, or curves in the beds.

"At the bathing-cots the limestone predominates, but very few fossils have been found in it, with the exception of the coral *Lonsdaleia*; but a little lower down the river the shales occur in thicker beds, and in several places, at least, are rich in fossils. The section is drawn from the highest of these broad bands of shale, where there is a cutting about 15 feet vertical, perpendicular to the



Section, about 15 feet in vertical height, not far below the Bathing-cots, on the River Hodder, Stonyhurst.
Reduced from a drawing to scale, by Wm. van Waterschoot van der Gracht, Esq.

a 1 *a*. Limestone forming roof of section, about 9 feet in thickness here; *g*, *g*. lenticular masses of chert forming irregular bands or nodules in the limestone; *b*, *b*. dark carbonaceous finely laminated shales (varying in thickness) separated by *a*, *a*, bands of limestone of variable thickness, some only a few inches; *d*, *d*. shales with Trilobites, etc., subdivided by *c*, *c*. thin seams of mixed limestone and clay in rectangular blocks; *f*, *f*. shales with "*Palæocoryne*," etc., separated by *e*. band of limestone, under which specimens of "*Palæocoryne*" were found.

Almost all the fossils enumerated were found in the upper thick band of shale *b*, under the beds 1, *a*, *g*.

It is estimated that the total thickness of beds observed here along the River Hodder exceeds 500 feet. Above are grits and fine sandstones (Longridge Fell).

dip of the strata (see section). There is a roof of about 9 feet of limestone, then some thin bands of limestone and shale, and below these several thick layers of soft shales, separated by thin seams of limestone about 2–3 inches thick. In the upper of these broad bands of shale most of the fossils have been found, with the exception of '*Palæocoryne*,'[1] which occurs somewhat lower down.

"The jointing of the shales is very clean cut, the horizontal lines being usually parallel, though the surfaces of the joints are not. There are also transverse joints likewise parallel. The cleavage is parallel to the surface of the stratum, and very greatly developed for 2 or 3 feet from the exposed parts.

"In the sketch may be seen two thin bands (*c*, *c*) at about 8 inches apart, traversing and dividing the layers of shale (*d*, *d*). These bands, about $\frac{1}{4}$ inch thick, are harder, from the presence of more lime, and are broken up into almost square blocks measuring $\frac{3}{4}$ inch each way. Most of the Trilobites were found about 1 inch above each of these bands, the other organisms, the Crinoidal ossicles and most of the shells in the same line or a little higher up. The rest of the shale is almost free from fossils, and is covered with minute flakes of mica. Altogether the shales are too brittle to allow us to get out large slabs. It is easy to see that the Trilobites, especially the smaller specimens, must have lived in groups together. The largest specimens are often 4 or 5 inches apart from the others. We cannot pretend to have fully examined the beds lower down, but until now no ferns, or traces of terrestrial life, have been found in strata nearer than 40 or 50 feet below this horizon."

Genus Phillipsia, *Portlock*, 1843.

General form oval; glabella with nearly parallel sides, marked by either two or three short lateral furrows; the posterior angles, forming the basal lobes, always separated by a circular furrow from the rest of the glabella; eyes large, reniform, delicately faceted; cervical furrow deep; free-cheek separated from the glabella by the axial suture, which forms an acute angle with the circular border of the cheek in front of the glabella; whilst the facial suture cuts obliquely across the posterior margin just behind the eye, leaving a small pointed portion fixed to the glabella by the neck-lobe; angles of cheeks more or less produced, margin of head incurved, forming a striated and punctated rim. Thoracic segments nine in number, the axis distinctly marked off from the side-lobes or pleuræ by the axal furrows; the abdomen or pygidium usually with a rounded border, the axis composed of from 12 to 18 coalesced segments.

1. *Phillipsia van-der-Grachtii*, sp. nov. Pl. XIV. Figs. 1–6.

This is a small, but very interesting form of Trilobite. The characters best observed are, the extremely long cheek-spines, more

[1] This form may prove to belong to quite another group of organisms. It is at present under examination by Dr. Hinde, who has kindly consented to study it microscopically.

than one-third the entire length of the animal; the presence of a tubercle on the axis of the neck-lobe of the glabella, and the rounded form of the pygidium. The border of the head-shield is semicircular, with a broad rounded margin; the glabella is somewhat expanded in front, and the facial suture curves away from the glabella outwards before it intersects the frontal margin. The eyes are rather large, but are only faintly shown in Figs. 4 and 5, not being well preserved in the soft shale. The axis of the thorax is broad, being rather more than a third of the whole breadth of the body, and is separated by a distinct and straight furrow from the pleuræ on either side. The body-segments, which are nine in number, are straight and smooth, and have rounded extremities to their pleuræ. The outline of the pygidium is rounded, with a smooth and narrow margin; the axis narrows rapidly to its extremity and appears to consist of about eight coalesced segments, but these are only faintly indicated. The extremity of the axis reaches nearly to the outer edge of the border of the pygidium. The two nearly entire specimens figured (Figs. 1 and 3) are each of them 12 millimetres in length.

Compared with *Phillipsia Eichwaldi*,[1] the species it most nearly resembles, *Ph. van-der-Grachtii* agrees with it in the possession of long cheek-spines, and in the presence of a median tubercle on the axis of the neck-lobe of the glabella. It differs from it in the absence of any furrows on the glabella. The segments of the cephalothorax agree with those of *Ph. Eichwaldi*, but the pygidium differs greatly in outline, and also in the number of coalesced segments. The margin of the pygidium in *Ph. Eichwaldi* is broad and smooth, and ornamented with fine parallel lines or striæ, but in *Ph. van-der-Grachtii* the border is narrow and much more rounded in outline. The axis of *Ph. Eichwaldi* shows distinct evidence of the coalescence of 16 segments, whilst *Ph. van-der-Grachtii* does not show more than 8 coalesced segments.[2] *Ph. Eichwaldi* measures 25 mm. in length, or double the size of *Ph. van-der-Grachtii*.

The only other species besides *Ph. Eichwaldi* which might be compared with *Ph. van-der-Grachtii* are *Ph. Leeii* and *Ph. minor*, from the Culm-measures of Waddon Barton, near Chudleigh, Devonshire, but the cheek-spines are not so long, and the pygidium is more pointed and consists of about 14 coalesced segments.

I have much pleasure in dedicating this species to Mr. William van Waterschoot-van-der-Gracht, an old student at Stonyhurst, who, as already mentioned, accompanied the Rev. G. C. H. Pollen in his geological rambles along the banks of the Hodder, and also collected many of the specimens here referred to.

[1] See Mon. Pal. Soc. Carb. Trilob., 1883, p. 22, pl. iv.

[2] It is probable that this interesting little species may hereafter prove to be an *immature form* of some larger Trilobite, at present unknown, from these beds. Meantime it seems more convenient to treat it as distinct, and to describe it as we have done. Numbers of this little form have been found on a slab in close proximity to one another.

2. *Phillipsia Polleni*, sp. nov. Pl. XIV. Figs. 7–12.

This species is considerably larger than the preceding one; the head is about one-third broader than long; the central portion of the glabella is smooth and rounded, gradually becoming narrower towards the front and being broadest at the neck-furrow. The facial suture runs in an undulating line between the glabella and the genal portion (free-cheek) of the head. The eyes, as a rule, are not well preserved, and in some specimens they might (as in the preceding one) be supposed to be wholly absent; but a careful study of numerous specimens shows that this is due, not to their absence, but to their imperfect state of preservation, the facets of the eye being clearly seen in Figs. 9 and 11. This condition of the eyes, due to compression, is also observable in the Trilobites from the Culm of Devonshire (see Mon. Pal. Soc. Carb. Trilobites, 1884, pl. x. p. 66). The contour of the head-shield is semicircular, the posterior genal angles are slightly produced, forming a short spine to each cheek (as seen in Figs. 8, 9, and 11). There are no furrows visible on the glabella. The neck-lobe is well developed and is broader than the free-segments which follow it; the pleural portion is less distinctly marked. The free-segments of the cephalothorax are nine in number; the axis is very slightly raised, and is nearly one-third broader anteriorly than the pleural portions, but diminishes in breadth gradually towards the pygidium; the extremities of the pleuræ are slightly pointed and recurved; the pygidium is one-third broader than long; the border is slightly raised, the axis being rounded off at some little distance short of the posterior border. The number of coalesced segments observable in Figs. 9 and 12 is about seven, but in Fig. 10 it apparently exceeds that number.

Compared with *Phillipsia Colei*, from the Carboniferous of Ireland, which is no doubt its nearest allied species (see Mon. Pal. Soc. Carb. Trilobites, 1883, p. 16, pl. ii.), *Ph. Polleni*, differs from it in the form of the glabella, and also in having the cheek-spines more pronounced, and in the absence of furrows on the glabella. The extremities of the pleuræ are rather more pointed in *Ph. Colei*. In the pygidium the border of the shield is not so wide in *Ph. Polleni*, and the axis is more rounded at the extremity and does not reach so near to the outer edge of the border as it does in *Ph. Colei*, in which the margin of the pygidium is also more corrugated. *Ph. Colei* is the only Carboniferous species which closely approximates to *Ph. Polleni*; but the differences just pointed out may easily suffice to distinguish them from one another. I venture to dedicate this form to the Rev. G. C. H. Pollen, S.J., of Stonyhurst College, to whom I am indebted for my knowledge of these Trilobites, and whose careful geological investigations brought about their discovery.

Both Trilobites belong to the Carboniferous Formation; but as to their *exact* geological horizon in the series it seems at present most unwise to speak dogmatically, owing to the difficult nature of the ground to be examined and the almost certainty of the

recurrence of the same beds brought in again by faults and folds. The absence of palæontological horizons to fix the position of other beds above and below, is also greatly felt. I have no doubt, however, that if Mr. Pollen is able to give the necessary time to the task, he will unravel the succession, so far as it affects the banks of the Hodder. For larger areas we may look with confidence to Mr. Tiddeman and the officers of the Geological Survey, to whom we already owe so much, for light and guidance.

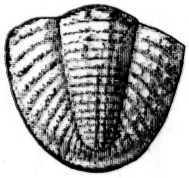
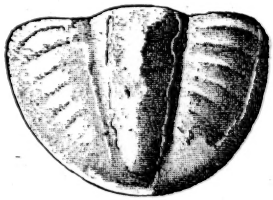
2.   1.

Fig. 1.—Pygidium of *Phillipsia Derbiensis*, Martin (× 3 times nat. size), in red Carboniferous Limestone; R. Hodder.

Fig. 2.—Pygidium of *Phillipsia gemmulifera*, Phillips, sp. (× 3 times nat. size), in dark greyish Carboniferous Limestones; R. Hodder.

Postscript.—In addition to the numerous remains of Trilobites from the carbonaceous shales of the Hodder bank, Mr. Pollen has sent me up two small pygidia (see Woodcut) obtained by him, Fig. 1 from a dark reddish pebble, Fig. 2 from a dark greyish pebble of crystalline limestone, obtained out of the river-bed itself. As these pebbles have undoubtedly come from points much higher up stream, and have undergone rounding, the exact position of the parent rock is not at present known.

Fig. 1.—The larger abdominal shield, or pygidium, in the red matrix measures 10 millimetres in length, by 13 mm. broad, and is composed of 13 coalesced segments, very indistinctly seen, being partly decorticated; the axis of the tail is roundly elevated, about 4 mm. broad at the proximal border, where it articulates with the cephalothorax and tapers down to an obtuse point close to the posterior border; the pleuræ composing the margin are indicated by distinct lateral furrows on the pygidium, around the margin of which there is a narrow smooth border 1 mm. broad. The surface is not ornamented by any tubercles or punctæ. I refer this form to *Phillipsia Derbiensis*, Martin (see H. Woodward's Mon. Carb. Trilobites; Pal. Soc. Mon. 1883, p. 12, pl. i. figs. 2a, 6, and 9) (see Woodcut, *supra*, Fig. 1). There is a minute *Goniatite* imbedded in the same pebble.

Fig. 2.—The second and smaller tail, preserved in a pebble of glistening crinoidal limestone, is 8 mm. in length and the same in breadth; it is composed of 14 coalesced segments, the ridge of each segment being ornamented by a single row of very minute tubercles; there is a very narrow smooth margin around the border of the tail-shield; the axis is about 3 mm. broad where it articulates with the cephalothorax; it diminishes posteriorly, and it terminates

bluntly within the margin of the shield (see Woodcut, *supra*, Fig. 2). I have no doubt in referring this specimen to *Phillipsia gemmulifera*, Phillips, sp. (See H. Woodward's Mon. Carb. Trilobites, Pal. Soc. Mon. 1883, p. 17, pl. iii. figs. 1–6.)

EXPLANATION OF PLATE XIV.

FIGS. 1–6. *Phillipsia van-der-Grachtii*, H. Woodw.

FIG. 1. Specimen showing part of head with long cheek-spines and entire body-segments and pygidium. One eye seen on right side of head. × $\frac{4}{1}$.

FIG. 2. A fairly well-preserved head-shield with long cheek-spines. × $\frac{4}{1}$.

FIG. 3. A nearly complete example viewed from the under side; the test is, however, removed. It gives the general proportions fairly well. × $\frac{2}{1}$.

FIG. 4. A specimen with the head and body-segments preserved, wanting the last two, and the pygidium also, with one long cheek-spine only preserved; eyes faintly seen. × $\frac{4}{1}$.

FIG. 5. Small specimen showing imperfect head-shield with eyes and cheek-spines, body-segments displaced and pygidium squeezed over them. × $\frac{4}{1}$.

FIG. 6. Imperfect specimen, showing head-shield and cheek-spines, eyes not preserved; body-segments obscure. × $\frac{2}{1}$.

FIGS. 7–12. *Phillipsia Polleni*, H. Woodw.

FIG. 7. Specimen showing glabella, body-segments, and pygidium, but wanting left margin and both cheeks. × $\frac{2}{1}$.

FIG. 8. An entire but poorly preserved specimen, giving, however, valuable details of head, facial sutures, body-segments, and pygidium. × $\frac{2}{1}$.

FIG. 9. Specimen nearly entire, the pleuræ injured on right side, showing eyes and facial sutures and angles of head-shield.

FIG. 10. A less perfect specimen, but with the test preserved. The pygidium is perfect, and has a larger number of coalesced segments composing it than the others. Possibly distinct?

FIG. 11. A detached free-cheek showing eye well preserved.

FIG. 12. A detached pygidium.

All the specimens are from the thin, laminated, black carbonaceous shales. Carboniferous formation: banks of the Hodder near the Bathing-cots below Hodder House; Stonyhurst. The "types" preserved in the British Museum (Nat. Hist.).

II.—PHYSIOGRAPHICAL STUDIES IN LAKELAND.

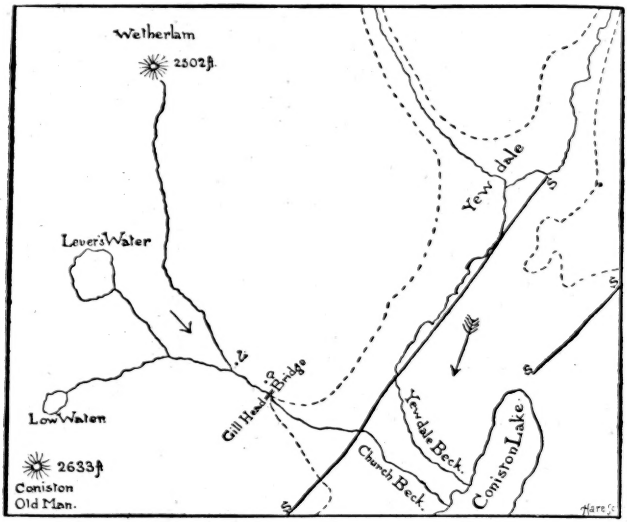
By J. E. MARR, M.A., F.R.S., Sec.G.S.

1. CHURCH BECK, CONISTON.

WHEN working amongst the rocks of Lakeland, one is often confronted with problems connected with the superficial features of the country, and as some of these are of more than local interest I propose to notice them from time to time.

Church Beck, flowing in a south-easterly direction from the eastern slopes of the Old Man range, passes through Coniston Valley to discharge its waters into the head of Coniston Lake, not far from the point of entrance of the more important Yewdale Beck, which flows through the wide valley of Yewdale in a southerly direction. Church Beck, on the other hand, flows through an upland valley, and is joined by the union of Low Water, Lever's Water, and the Red Dell Becks. Walking up its banks from Coniston Village, one presently comes upon an exposure of the Skelgill graptolitic shales at a height of about 225 feet above sea-level. Above this, one rapidly mounts the hill, and passing a

considerable waterfall, reaches Gill Head Bridge at a height of about 540 feet. Above this is an alluvial flat extending to the Coniston copper works at the junction of the Red Dell Beck with that formed by the union of Lever's Water and Low Water Becks. The bottom of this alluvial valley is occupied by stratified deposits, formed by the washings of the copper works, which will, of course, not be confused with the much older deposits about to be described.



Explanation of Map of Church Beck, Coniston.

SS, Outcrop of Skelgill beds (shifted by fault above head of Coniston Lake).
a, Position of gravels with fragments of Skelgill beds.
b, Position of contorted loam, etc., on Boulder-clay.
The dotted line on map marks the 500-feet contour line.
The plain arrow indicates the direction of the Local Glacier.
The feathered arrow indicates the direction of the Yewdale Glacier.

Close to Gill Head Bridge, by the side of the road on the left bank of the stream, is a section in gravel and loam (a similar gravel occurs by the side of the road at a lower level and nearer Coniston Village at a height of 400 feet).

These deposits are well stratified, and rest on glaciated rocks with striæ pointing down the Church Beck Valley, and no doubt formed by a glacier coming down from the Old Man Range. Amongst other stones, they contain a number of fragments of the Skelgill graptolite-bearing beds, occurring *in situ* about 350 feet lower down the valley. It is of interest to inquire how these fragments got into this position.

The top of the gravel forms an indistinct terrace, at a height of about 575 feet, which may be seen on either side of the valley; but it does not run continuously round it, for the stratified deposits abut against and overlie Boulder-clay near the top of the alluvial plain, just below the Copper Works, and by the side of the above-

mentioned road. Here an interesting section is seen of extremely fine laminated loam, resting upon the Boulder-clay, which, however, contains seams of a similar loam. This appears to be the section drawn in Mackintosh's "Scenery of England and Wales," p. 393, though all the details shown there are not now visible.

These stratified deposits are of such a nature that they would be unhesitatingly referred to "mid-Glacial gravels" by some writers; but on this view the presence of the fragments of Skelgill Shale will be difficult to explain. It is true that the Skelgill Beds occur at a level higher than that of the gravel both to the north-east and south-west of Church Beck, but they certainly cannot have been brought by ice from the south-west, for the movement of the ice was generally from the north, nor could they have come from the north-east, for the only place where they occur there at a higher level than that of the gravel, is on the opposite side of the Yewdale Valley, down which the main mass of ice moved in a general southerly direction. The local distribution of the gravel is an objection to their having been brought by floating ice in a glacial sea. If they had been so brought, we should expect to find similar deposits widely spread on the low ground about Coniston Lake, but they do not occur there. How, then, did they reach their present position? The most plausible explanation that I can give is as follows:—

It has been seen that the local ice from the Old Man Range once came down the valley below Gill Head Bridge, as shown by the rock striations. This glacier had certainly receded above that bridge when the stratified deposits were formed. But the more important mass of ice coming down Yewdale would extend across the mouth of the Church Beck Valley, blocking it up, and converting it into a glacier-dammed lake. Into this lake the coarse sediments would be washed off the hillsides to form the gravel terrace. The presence of fine loam higher up the valley, and the absence of the terrace just above, suggests that at this time the local glacier was about the head of the lake, and the muddy waters from it were discharged into the lake to form the impalpable laminated loam. The contortion of the loam, and the inclusion of patches of loam in the Boulder-clay, point to slight oscillations in the glacier, which at one time retreated from the lake shore, at another advanced a short way into the lake. The main Yewdale glacier would shed its bergs into the lake, as does the Aletsch glacier into the Märjelen See, and fragments of the Skelgill Beds carried in the ice could in this way be floated up and incorporated in the gravels. This certainly seems the most natural explanation of the phenomena exhibited in this valley. The accompanying map, when looked at in connection with a general map of the district, will show how favourable were the circumstances for the blocking up of the valley by ice.[1] The 400 foot gravel appears to belong to a lower terrace, formed when the Yewdale glacier had shrunk.

[1] The top of the terrace does not coincide with any *col*. The surplus water from the lake probably disappeared through rents in the glacier.

I should not have noticed this case were it not for the tendency of some writers to assign all stratified deposits connected with Glacial beds to a mid-Glacial period. The stratified deposits of Lakeland and its neighbourhood are very numerous, but their modes of production are very diverse, and after many years of work in the district I have seen no proofs of an extensive mid-Glacial submergence there; everything points to the contrary.

III.—Notes on the Osteology of *Ancodus* (*Hyopotamus*).

By Prof. W. B. Scott, F.G.S.

Professor of Geology in the E. M. Museum of Geology, Princeton, New Jersey, U.S.A.

It might well appear that the labours of Kowalevsky and Filhol had so completely determined the structure of this genus, that nothing of importance remained to add to their results; but this is very far from being the case. The European material, found principally at Hempstead and at Ronzon, near Puy in France, is so scattered that its proper association is a matter of great difficulty, and errors are unavoidable.

Mr. J. B. Hatcher, Assistant in Geology, and one of the Curators of this Museum, has during the past two summers been engaged in collecting from the Oligocene beds of South Dakota, and among many other treasures has obtained several all but complete skeletons of *Ancodus*, belonging to the species which Osborn and Wortman have lately named *A.* (*Hyopotamus*) *brachyrhynchus*. This material proves to be of great interest and importance, and will be fully described in an illustrated monograph which is soon to appear. The object of the present preliminary notice is to call attention to some of the more striking characteristics of this curious animal.

The American species of *Ancodus* present certain constant differences from the European members of the genus. (1) The muzzle is not drawn out into such an extraordinary rostrum; (2) The skull has greater vertical height, though this difference may be due in part to the crushing which the Ronzon specimens have undergone; (3) The coronoid process of the lower jaw is much more prominent and more decidedly recurved.

The structure of the cranium is very similar to that found in the peculiar American genus, *Oreodon*, the proportions of the cranial bones being nearly identical; but while in *Ancodus* the facial region is remarkably elongate, in *Oreodon* it is extremely short, the teeth forming a continuous series without diastemata.

The vertebral column is also oreodont in character, and the odontoid process of the axis, as in that family, is neither conical nor spout-like, but half way between the two. The ribs are long and broader and more flattened than in the oreodonts, and the sternum is similar to that of the latter family. The scapula, humerus, ulna, and radius differ from those of the oreodonts only in detail; thus the ulna is less reduced and has a shorter and remarkably heavy olecranon.

The manus is pentadactyl, with a large, well-developed pollex.

In 1884 I showed that *Oreodon* possessed a pollex, the first artiodactyl in which this structure had been observed; later I found it in the Uinta (Upper Eocene) form *Protoreodon*. *Ancodus* is thus the third genus known to possess it, and is doubtless an indication that the Eocene artiodactyls will be generally found to have five digits in the manus. In this genus the pollex is larger relatively than in either of the others, and must have been of some functional value.

The femur and tibia differ from those of *Oreodon* only in their much greater size and massiveness; they are longer and heavier in proportion to the fore-limb than in that genus, but the shape and character of these bones are similar. The fibula is complete, but the shaft is much reduced and the distal portion is shifted more beneath the tibia than is common among artiodactyls of such antiquity.

The tarsus is oreodont in type, with some curious specializations, which are more elaborated in the American species than in those figured by Kowalevsky and Filhol. The astragalus and calcaneum are interlocked in a remarkably complex and perfect way, which, however, can hardly be rendered intelligible without the aid of figures. As in the oreodont, and indeed most artiodactyls, the ecto- and mesocuneiforms are ankylosed into a single piece. The entocuneiform is very unusually large and heavy, and to its distal end is attached a good-sized nodule representing a rudiment of the first metatarsal. The artiodactyl hallux was first demonstrated by Osborn and Wortman, who found it in the foot upon which they established the genus *Artionyx*, but which I have shown to be referable to *Agriochœrus*, Leidy, a most remarkable artiodactyl with claws instead of hoofs. Marsh has lately published the statement that a rudiment of the hallux is preserved in *Protoreodon* (*Eomeryx*).

The phalanges of *Ancodus brachyrhynchus*, and especially the unguals, are quite different from those of the European species and approximate more to the oreodont type.

The general resemblance of *Oreodon* and *Ancodus* has been commented on by many writers, but we are now for the first time in a position to determine just what that resemblance is, and we find it to extend to all parts of the structure. When the Upper Eocene genus *Protoreodon* is taken into account the similarity is found to be all the greater, for the latter has the oreodont peculiarities less markedly developed and still retains the pentaselenodont type of dentition. *Agriochœrus* is also clearly related to the oreodonts, but represents a very aberrant line. We may infer with considerable confidence that *Oreodon*, *Ancodus*, and *Agriochœrus* belong to three divergent branches of the same stock. It is true that the former is an exclusively American genus, while *Ancodus* very probably originated in the Old World, reaching North America by migration in early Oligocene times. Doubtless, however, we shall eventually find a Middle Eocene form, common to both hemispheres, which will prove to be the ancestral type required.

IV.—Chloritic Marl and Warminster Greensand.

By C. J. A. Meyer, F.G.S., and A. J. Jukes-Browne, F.G.S.

In the year 1878 there was a short controversy between us respecting these beds in the pages of this Magazine. Since that date we have both enlarged our knowledge of the beds at the junction of Chalk and Greensand, and recently we have had an opportunity of meeting at Seaton in Devon, and of examining the Whitecliff and Beer Head sections together. The result of this and of friendly discussion is that we have both modified our views and have come to an agreement on the most important points about which we differed in 1878. Under these circumstances we think it will save trouble and facilitate future enquiry if we jointly express our present views on the subject so far as we are in agreement.

1. *Chloritic Marl.*—As far as we can ascertain, this name was first applied to certain beds at the junction of the Chalk and Greensand in the Isle of Wight.[1] It was subsequently adopted by Prof. E. Forbes, for a bed which occupied a similar position in Dorset, and has since been used for any bed lying at the base of the Chalk Marl and containing fossils and green grains.

It does not follow, however, that the basal part of the Chalk Marl is everywhere of exactly the same age; there may have been continuous deposition over one part of the sea-floor, while a neighbouring part was swept by a strong current. We think this was the case, because in some localities there is a complete passage upwards from Greensand to Chalk, while in others there is a clearly marked break and plane of erosion between them. Where there is a passage the beds with phosphate nodules are probably on the same stratigraphical horizon; but wherever a break occurs the basement of the Chalk may belong to a higher horizon.

In the Isle of Wight, in N.E. Dorset, and South Wilts there is either complete passage or very slight signs of erosion; there is always soft greensand below the bed with phosphatic nodules, and the latter always contains the sponge *Stauronema Carteri*.

In the western part of Dorset there is always a break between Greensand and Chalk: the top bed of the Greensand is generally a hard calcareous sandstone; the nodule bed at the base of the Chalk does not contain *Stauronema*, but yields *Scaphites æqualis* in abundance, whereas in the Isle of Wight *Scaphites* only occurs in the Chalk Marl about 20 feet above the base.[2] Consequently we consider that the *Scaphites* bed of Dorset belongs to a higher stratigraphical horizon than the *Stauronema* bed of the Isle of Wight.

Now if the name Chloritic Marl is to be retained—and it seems to die hard—we think it should be restricted to the horizon or

[1] Ibbetson, "Notes on the Strata of the Isle of Wight." London, 1849. 8vo.
[2] It has been recorded from the Chloritic Marl, but we believe that this is a mistake; at any rate, the find has never been confirmed.

"hemera" of *Stauronema Carteri*, and that the name should cease to be applied to the *Scaphites* bed of Dorset and Somerset, or to any bed in Devonshire. We are now of opinion that the bed which lithologically resembles Chloritic Marl at Beer Head, namely, No. 13 of the succession described by one of us in 1874,[1] is really the representative of the zone of *Belemnitella plena*, for it contains that fossil and passes up into the basement bed of the Middle Chalk.

2. *Warminster Beds.*—As one of us pointed out in 1878,[2] the fossils of the Warminster fauna were always collected from the surface of a field near Chute Farm; but there is now an exposure of the Greensand which contains these fossils in a small pit at the neighbouring farm called Rye Hill (Ray Hill on the old Ordnance Map). This Greensand is seen to pass up into a glauconitic marl containing scattered phosphatic nodules, and in this nodule-bed at another exposure *Stauronema Carteri* occurs, the bed gradually passing up into Chalk Marl. Hence it is clear that the home of the Warminster fauna is entirely below the Chloritic Marl, if that be defined as the horizon of *Stauronema Carteri*.

It is, moreover, important to note that the fossiliferous greensand does not contain brown phosphatic nodules, only a few pale yellow calcareous concretions; consequently fossils in brown phosphate purporting to come from the "Warminster Greensand" must really have come from the overlying Chloritic Marl, and should therefore be excluded from the list of Warminster Greensand fossils. We find that many such phosphatic fossils occur in the collections of different museums, and this fact has no doubt tended to increase the confusion between the Warminster bed and the Chloritic Marl.

On the other hand, in the Isle of Wight the bed of soft greensand which lies below the nodule-beds, and which has by some been included in the Chloritic Marl, contains *Terebratella pectita*, *Catopygus columbarius*, and other Warminster species, which do not occur in the *Stauronema* beds above.

It is true that in Devonshire (Beer Head, etc.) a Warminster fauna is present in beds which appear to be wholly above the Upper Greensand; this, however, is probably due in part to the survival of shallow-water species in the neighbourhood of a coast-line, for *Pecten asper* and other Warminster fossils are there associated with *Holaster subglobosus* and *Ammonites Mantelli*, which are essentially Chalk Marl species.

We think, therefore, that the Warminster Greensand and its equivalents should be regarded as the summit of the Upper Greensand, and the *Stauronema* bed or Chloritic Marl as the lowest horizon which can be included in the Chalk, this horizon being sometimes absent through non-deposition.

These views will entail a complete revision of the lists of Chloritic Marl fossils, as well as of the Warminster greensand fossils.

[1] Quart. Journ. Geol. Soc. vol. xxx. p. 369.
[2] Geol. Mag. Dec. II. Vol. V. pp. 547–551, December, 1878.

V.—Mr. Harker and Mr. Deeley on the Scandinavian Ice-Sheet.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

MR. DEELEY tells your readers that he has recently been to the summit of Mont Blanc, and has been studying the difference between *névé* and glacier ice. This is interesting; but we thought that a great many people had done the same thing during the last hundred years, and we thought that one of them, Forbes, had studied the famous Mountain and the phenomenon in question to good effect, not in a casual visit to the Alps, but in the course of many years of patient labour. Among other things we also thought he had shown that in a viscous body like ice, the slope of the upper surface necessary to make it begin to move is the same as the slope which would be required to induce motion in the ice if its bed were inclined at an angle. He further collected considerable evidence to show what the least angle is upon which ice will begin to move. This is the slope, *the least slope*, available. It is nothing less than astounding to me that anyone should venture to postulate a Scandinavian ice-sheet in the North Sea until he had considered this necessary factor, and how it would operate.

The Scandinavian ice-sheet was, I believe, the invention of Croll, who, sitting in his arm-chair and endowed with a brilliant imagination, imposed upon sober science this extraordinary postulate. He did not dream of testing it by an examination of the coasts of Norway, or even of Britain, but put it forward apparently as a magnificent deduction. All deductions untested by experiment are dangerous. Thus it came about that the great monster which is said to have come from Norway, goodness knows by what mechanical process, speedily dissolved away on the application of inductive methods. *Of course* it still maintained its hold upon that section of geologists who dogmatise in print a great deal about the Glacial period before they have ever seen a glacier at work at all; but I am speaking of those who have studied the problem inductively. First Mr. James Geikie, a disciple of Croll, was obliged to confess that this ice-sheet, which is actually said to have *advanced* as far as the hundred-fathom line in the Atlantic, and there presented a cliff of ice like the Antarctic continent, never can have reached the Faroes, which had an ice-sheet of their own. Next Messrs. Peach and Horne were constrained to admit that no traces of it of any kind occur in the Orkneys, or in Eastern Scotland. They still maintained its presence in the Shetlands; however, this was upon evidence which is somewhat extraordinary. I do not mean the evidence as to the direction of the striation, which was so roughly handled by Mr. Milne-Home, but I mean the evidence they adduce that the boulders found on the islands are apparently all local ones, and that, contrary to the deposits of glaciers, they diminish in number as we recede from the matrix whence they were derived.

If we cross the North Sea we have the converging evidence of Bonney and Matthew Williams in the Lofodens, of Pettersen in Central Norway, and, may I add, of Kjerulf, whose splendid map of

the distribution of striæ and boulders in South Norway is most instructive, all of them plainly showing that such an ice-sheet is an impossibility unless we are to ignore the facts, for the Norwegian ice is shown not to have even extended to the islands on its coast. All this, and a great deal more, I have called attention to at very great length elsewhere; and I am bound to say I felt surprised that Mr. Harker, whose laurels have been won as a petrologist, should have ventured into the very intricate and difficult region of glacial geology without ascertaining what the best men (I mean the men I quote) had already done, and should not have felt some hesitation in basing so stupendous a postulate as a North Sea ice-sheet upon the occurrence at Dimlington of some stones like the rocks found in Viken.

Mr. Harker complains that I have travestied his argument by converting it into a simple syllogism. That argument is in print, and any of your readers is at liberty to construct a more attractive syllogism if he can. The fact is, some arguments look like travesties when thus analysed. Apart from this, Mr. Harker would oblige me by re-stating what he actually meant when, in view of the postulate admitted by him and Mr. Lamplugh, that more than nine-tenths of the Yorkshire boulders came from the north-west, he adduced as *a priori* probable that the other tenth came from the north-east, or rather the east.

He misunderstood me if he supposed I questioned his identification of the rocks in question. As a petrographer he is my master, and a very accomplished master; when he wanders away into glacial geology he has not the same vantage. When I said that in my rambles in East Anglia, and on the coasts of Durham and Yorkshire, I had not myself met with these so-called Scandinavian stones, I meant not that such stones do not occur there, but that they are distributed very locally, which they are. In referring to another petrologist it was for the purpose of saying that we must be very certain that we have exhausted every possibility of these stones having come from the Cheviots, or some other British site, before we take the course of going to the Cattegat for them; and that, inasmuch as the vast mass of the boulders found with them came from the north-west, it is, *primâ facie*, probable that they also may have come from some buried or undiscovered dyke or outflow in Britain, more especially as the Laurvig rock is itself so very local in Norway. Assuredly this is a reasonable position to take.

Supposing, however, they come from the Cattegat—the next question is, how did they come? That was, and remains, the really interesting question between us. I argued (in view of the facts already mentioned) that a Scandinavian ice-sheet is an impossibility. At all events, if it be possible, the facts collected by some of the most experienced geologists, Norwegian and English, have to be explained. Mr. Harker has ignored them all, and inasmuch as it was he who was mainly responsible for this correspondence, I think it would be welcome to us all if he were to suggest some answer to them.

If an ice-sheet crossing the North Sea is not possible as the porter of these stones, how are we to account for them? Icebergs seem to me to be as difficult to explain on many grounds as an ice-sheet. *Inter alia*, I presume the postulated ice must have collected the Carboniferous stones from Durham, and the stones from the Cheviots, where we have no evidence of continuous submergence. I therefore suggested the possibility of these local deposits of northern stones on the seaboard, all of which, so far as I have seen, *are rounded and water-worn*, and not one of them has the flattened parallel sides of undoubted glacier stones, being possibly ballast, which finds its way into many strange places.

This very year I heard, at Southwold, a most circumstantial account of two cargoes of stones from wrecks which had been scattered on the beach—one of them, a steamer from Guernsey with Guernsey rocks, wrecked near Aldborough, and the other a cargo of paving-stones from Scotland. Mr. Hulke writes to say that he remembers a cargo of Elephants' tusks being similarly shipwrecked in the Channel. I further pointed out that the Norse pirates, whose ships were being continually wrecked on the Yorkshire and East Anglian coasts, curiously enough chiefly came from Laurvig and its neighbourhood, and not only must have carried ballast, but also used stones for anchors. This explanation does explain simply, completely, and absolutely the local character of the finds, and the occurrence of the stones on the strand. It is on the strand the great bulk of them occur. Mr. Harker says, however, that some also occur *in situ* in the cliffs. He does not say he has found any himself. If he has, and the question is very important, its importance has certainly been overlooked in his former papers, which I quoted. The question is not a simple one to decide, and we should all be grateful for precise and definite details, and not *obiter dicta*.

It is very difficult sometimes on these coasts (and I confess to having been myself misled the other day at Cove Hithe) to distinguish between boulders actually in the clay originally and those driven into the face of the cliff by the high tides. The safest test is to examine pits in the Boulder-clay some distance from the sea. Has this been done? Any details as to the exact facts of the finds other than on the strand would be very useful.

The issue is too interesting and too important to be left in doubt, and now that the question has been raised in this way it ought to be properly sifted, and by no one better than by Mr. Harker and Mr. Lamplugh. The stones in question deserve to be more systematically collected, and their external appearance as well as their internal structure described, and the provénance of each properly marked. Such a collection ought to be found in Jermyn Street, the best of Geological Museums, but, unfortunately, the Yorkshire boulders are badly represented there. I could only find two small specimens of the rhomb porphyr. I am bound to add that from my own superficial examination, the Yorkshire stones seem to differ both in colour and in the shape and size and mode of occurrence of the imbedded

felspar crystals from specimens of rhomb porphyry from Laurvig in the Natural History Museum.

Now for two personal matters:—Mr. Deeley says I have misquoted two letters of Professors Bonney and Hughes. The letters were private letters, written to myself, and are on my table. How anyone but myself and the writers can know what they contain I know not. All I know is that Prof. Bonney, in regard to the Lofodens, and Prof. Hughes, in regard to the shipwrecked stones on the East coast, absolutely confirm my statements of fact. The inferences are my own.

Secondly, Mr. Deeley says I sneer at official geologists. In my case to do so would be like parricide and fratricide. I have learnt a great deal of what I know from them, and have received unbounded courtesy from them. What I do object to, and shall continue to protest against, is the notion that geologists, official or otherwise, any more than any other scientific men, have a right to publish and discuss great issues *until they have read what other people have written about them.* It does further seem preposterous to me that a number of men should be told off to map so-called glacial deposits, and to write memoirs—*not* on the facts, but on a glacial explanation of the facts—who have never studied the mechanics of ice in the laboratory, and, what is more strange, have never seen a glacier at all.

Lastly, I suppose no other science but long-suffering geology would tolerate the absurdity—may I say the impertinence?—of a public advertisement from a casual visitor to Switzerland, that, having gone to Mont Blanc in the year of grace 1894, he proposed to settle the great question of the different action of *névé* and ice. Why, the question is a century old, and there was a whole library written upon it before either Mr. Deeley or myself was born!

VI.—The Yumuri Valley of Cuba—A Rock-Basin.

By J. W. Spencer, M.A., Ph.D., B.A.Sc., F.G.S.

NEAR the city of Matanzas, in Cuba, there is a beautiful valley called the Yumuri, of which the good people of the region are justly proud. Its interest to the geologist is unsurpassed in the island. At its entrance there is the most complete section of Tertiary rocks observed by me in Cuba. This valley is a record of the great erosion of the land during most of the Pliocene period, at the close of which it was partly refilled. The valley was re-excavated during the earlier days of the Pleistocene period, and suffered other changes, but it is the closing of the valley into a rock-basin in almost modern days of which I write.

Upon the northern side of the ridges, of which Pan de Matanzas is the highest point (1277 feet above tide), there are the remains of a former plain, which extended five or six miles to near the sea-shore, with an elevation of about 450 feet. Out of this plateau the Yumuri Valley has been excavated with a breadth of about three miles, and length of five or six miles, with a further rugged extension of one

of the tributary valleys to the foot of Pan de Matanzas, as shown on Map (Fig. 1). The floor of the basin rises from near tide-level to considerable elevations in its upper part, where there are low ridges produced by the unequal washings of the tropical rains. The floor of the lower part of the basin has been silted up so as to be

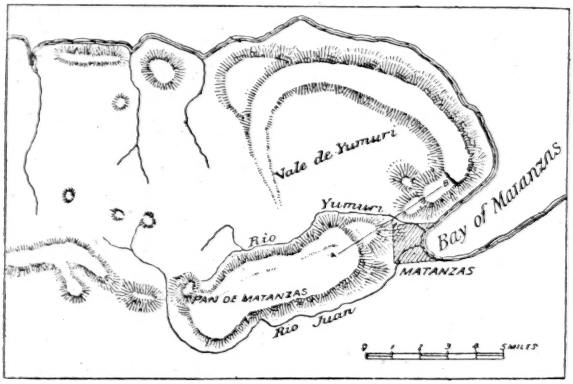


Fig 1

FIG. 1. Map of the Yumuri Valley, A B, position of section shown in Fig. 3.

level, and it is bounded by steep wave-washed banks indicating its former lacustrine character. The floor of the basin is underlaid by Cretaceous sands, decaying serpentine rock, or Tertiary limestone, which have been very extensively removed so as to expose the other and older named beds. The original valley was excavated out of the upturned beds of Miocene and Eocene limestones. During the following submergence, at the close of the Pliocene period, the valley does not appear to have been completely filled with the later calcareous rocks (Matanzas limestone of the author[1]), which contain, mostly, living organisms. During the long epoch of the earlier Pleistocene elevation and erosion, the Yumuri Valley was again excavated, and in some parts enlarged, so that only fragments of the Matanzas limestones are found on the sides of the valley. The south-eastern end of the basin is about three miles wide, and is divided by a hill into two lobes. The basin is cut off from the bay (or rather fjord) of Matanzas (with an increasing depth of from 1000 to 1500 feet) by a barrier ridge, whose base is about a mile wide, and whose height is from 250 to over 450 feet above tide (see Fig. 3). The Miocene and Eocene strata dip at from 20° to 30° S. 20° E., whilst the overlying Matanzas marls dip at 10° or 12° N. 20° E.; and these last are succeeded by modern coral-reefs on the seaward side of the ridge. The outlet of the broad valley is a *cañon* with vertical walls (below and sloping above) rising 250 feet above the water, and with a breadth of only 300 feet, like many other

[1] Described in a paper read before the Brooklyn Meeting of Am. Assoc. Ad. Sc. as an advanced notice of an unfinished paper.

recent *cañons* along the streams in Cuba. After seriously considering the untenable hypothesis of the origin of the basin as due to solution, I found that the explanation of the basin with modern insignificant outlet was due to recent dislocation, as was shown in the fault exposed in the longitudinal section of the *cañon*. The

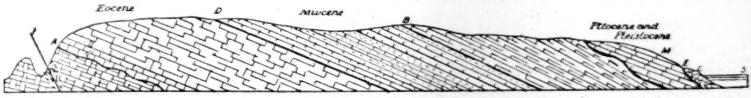


Fig 2

FIG. 2. Longitudinal section through the *cañon* of the Yumuri, at right angles to the strike; S, sea-level; C, raised coral-reefs; M, Matanzas limestones; B, slight unconformity between Miocene limestones and Miocene sands; D, base of Miocene; A, undulations in Eocene limestones; F, fault whose dip is 60°, general dip of strata varies from 20° to 30°.

Matanzas limestones have a thickness of about 150 feet; the Miocene 790 feet; and the Eocene above the fault 760 feet, but after making allowance for the dislocation along the fault, the total thickness of the Eocene may reach to 1200 or 1500 feet in the section along the side of the valley beyond the limit of Figure 2. The vertical elevation produced by the fault varies from 250 to nearly 400 feet, as shown in Figure 3. The valley had formerly extended in two lobes round an island to the Matanzas Bay, but

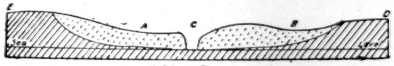


Fig 3.

FIG. 3. Section across the end of Yumuri Valley (same as section A B, Fig. 1). Broken shading represents the barrier raised in front of the valley; A and B, the former extensions of the two lobes of the valley; and C, the site of the *cañon*. The broken shaded section is about three miles long, and the maximum height about 450 feet.

with the elevation of the ridge to the named height the basin was produced. The plain of uplift has been preserved and exposed in the fault, shown at the inner end of the *cañon*, or on the side of the barrier-ridge facing the basin. The plain of the fault dips 60° with the adjacent strata crushed, the only structure of the kind seen in the whole section.

The discovery of the fault settled the origin of the basin, and the occurrence of late Pliocene beds in the uplifted mass brings down the date of the dislocation, after the erosion of the valley, into the later part of the Pleistocene period, or later, as would be suggested by the newness of the walls of the *cañon*.

After the earlier Pliocene elevation, there was a subsidence somewhat below the present altitude of the land, when the Zapata (mid-Pleistocene) formation was deposited.[1] At other places in Cuba these accumulations obstructed the drainage of the earlier Pleistocene valleys, and caused streams to cut across the last made

[1] This formation was described at the same time as the Matanzas.

limestones, as the land rose from the Zapata subsidence, although it has subsequently been slightly depressed again. These *cañons* are all alike, and date only from the elevation following the mid-Pleistocene deposits constituting the Zapata and gravels, and perhaps marls, of about the same date as the Columbia formation of the American coastal plain.[1]

There are other basins in Cuba where subterranean passages carry off the drainage, and those amongst the mountains are more striking than the numerous lime-sinks of Florida. But the Yumuri basin would have defied explanation had its outlet followed some other course than across the faulted ridge (which had barred the ancient drainage), and not exposed the dislocation. Some of the harbours along the extended coast may be found to be of the same origin as the Yumuri basin, and some of the valleys, such as that of Trinidad on the southern side of the island, have been more or less affected by late faulting.

VII.—A Reply to Sir H. Howorth's Paper on "Recent Changes of Level."

By Mark Stirrup, F.G.S.

In a paper published in the Geological Magazine, September, 1894, Sir Henry Howorth expatiates on recent changes of the relative level of land and sea in support of his views on the Mammoth age and his diluvial catastrophe, in which there seems to me some very extraordinary confusion in the matter of geological chronology and sequence of events. The first paragraph reads as follows:—"In some recent papers published in the Geological Magazine, I have endeavoured to show that at the *close* of the Mammoth age there was a very considerable dislocation of the Earth's crust, and that a consequence of it was the upheaval of some of the highest masses of land on the earth, including the massive mountains of Asia and the American Cordillera. I now propose to show that (as is *a priori* probable) there was a concurrent collapse or sinking of the ground over large areas, which, as in the corresponding upheaval, was *very rapid*, if not *sudden*" (the italics are mine). The suggested relationship of these various events and their alleged catastrophic character, induces me to again enter this ever-expanding field of controversy.

In support of his thesis Sir Henry first refers to the subsidences which resulted in the separation of England from the Continent, and consequent extinction of the Mammoth.

Assuming that the course of things was as stated, when it is further suggested that this event was *contemporaneous* with great dislocation of the Earth's crust, resulting in stupendous upheavals of mountain ranges in Asia and America, he attempts more than can well be proved. Sir Henry proceeds, in the development of his argument, to the evidence offered by the remains of submerged forests which exist at various points around our coasts, and attributes their

[1] The Lafayette Formation, by W. J. McGee, 12th Report U.S. Geol. Survey.

occurrence to a widesp red submergence, which extended to continental shores. He assigns the date of this event to a time immediately after the destruction of the Mammoth.

The evidence, I take it, of these submerged forests is far from corresponding with the interpretation thus given; for not only are they of different ages, but, instead of supporting any argument for a sudden or rapid sinking of the land, they seem clearly to indicate a *progressive* subsidence, marked by the distinct beds of peat, clay, sand, or gravel by which they are often accompanied. The changes of the level of the land which these submarine forests indicate no one would attempt to dispute; but to affirm their contemporaneous growth, or attempt to synchronize them with the end of the Mammoth period for the purpose of sustaining some fancied theory, is scarcely permissible or scientific.

That the peat and forest beds took a long time for their accumulation and growth might be proved from many of the instances cited by Sir Henry, having regard to the thickness of the deposits and the character of the plant remains, which, in some instances, indicate climatic changes of considerable duration.

That some of these sunken forests are of an age far more recent than that of the Mammoth, is shown by the prehistoric and Roman remains that have been occasionally extracted from them.

The difficulty attending the examination of submerged forests prevents researches which might prove of great interest with regard to their history, therefore those claim special attention which have been exposed to the light of day, such as those due to the dock excavations at Hull and Grimsby. A concise account of the sections is given in the memoir of the Geology of Holderness, by Mr. Clement Reid. Dr. Foster, writes Mr. Reid, thus describes the beds exposed in the Albert Docks at Hull:—"In the cuttings at the east end the upper stratum is silt, ... and immediately under the silt, the trunks, roots, and branches of Oak trees, together with a peat soil of two feet in thickness, beneath that a strong clay soil, and under this an extensive bed of blue sand, etc."

"At a depth of 40 feet below the level of the adjoining land, trees (chiefly Oak) are found in all positions; those which are upright and still *in situ* having been broken off within three feet of the roots. One Oak tree, of noble dimensions, is perfectly straight, its trunk being 45 feet long, and in the thickest part measuring 12½ feet in circumference; it is tolerably sound, but blackened in colour."

"The trees . . . . would require at least 300 years to attain the dimensions given."

A section taken during the excavation for the Alexandra Docks, two miles lower down the Humber, exposes the "Submerged Forest" at upwards of 20 feet below the level of mean tide. Messrs. Wood and Rome (Quart. Journ. Geol. Soc. vol. xxiv. p. 146) state that "the borings at Grimsby Docks disclosed the forest-bed at still greater depths than at Hull, varying at from 35 to 52 feet below high water . . . . In some parts there were two forest surfaces, divided by a bed of leafy clay from 5 to 15 feet thick."

Examples, such as those that I have cited, might readily be drawn from other localities, showing periods of repose and gradual growth rather than a sudden or rapid sinking of the land; and proving the utter fallacy of the doctrine which these sunken forests are presumed to support,—of a sudden or rapid sinking of the ground at that vague and mysterious date, the close of the Mammoth period.

After a quest over Europe and Asia in search of evidence for great uplifts of land and their complementary depressions, Sir Henry turns his attention to America, and in the ancient channels, now submerged beneath the sea, of rivers like the Mississippi, the Hudson, and the St. Lawrence, he finds so-called examples of the great catastrophe for which he is pleading.

He quotes extracts from papers by Professor J. W. Spencer and other well-known American geologists with regard to stupendous continental elevations and great subsidences. These enormous changes of level are facts well recognised by geologists; but the point is—Do any of the writers whose views are cited claim these earth movements to have taken place at the special geological period required by Sir Henry's postulate, or do they even assert those changes of level to have been either sudden or catastrophic.

The authors quoted would, I believe, be the first to repudiate the extraordinary inferences which Sir Henry seeks to draw from the facts. For instance, I will take the paragraph in which Sir Henry Howorth cites Prof. Spencer's remarks on the submerged valleys of the Mississippi and the St. Lawrence. Prof. Spencer says, in reference to the Mississippi:—"This valley is now submerged to a depth of over 3000 feet, and is the representative of the channel of the ancient Mississippi river, towards which it heads."

This quotation, and others of like character, are brought forward by Sir Henry as supporting his argument of a sudden and cataclysmic subsidence. Bearing this in mind, remark what Professor Spencer says in relation to this same submergence, quoted from the same paper as that from which Sir Henry has drawn his facts. "The length of time required to excavate the channels of these great rivers commenced as far back as the *Palæozoic* days. However, the culmination of that of the Mississippi was not until in the later Tertiary, before the Pleistocene period. As the St. Lawrence, now submerged to a depth of over 1200 feet for a distance of 800 miles, is mostly cut out of rocks of the Palæozoic group, except a belt of the Triassic, its antiquity must be very great. The culmination was also probably in the later Tertiary era, like that of the Mississippi, and the channels on the Californian coast, for there are submerged Tertiary rocks off the coasts of Massachusetts and Newfoundland at elevations much higher than the beds of the old channels." Prof. Spencer, moreover, does not argue that these submerged valleys or depressions in the Earth's surface are wholly due to terrestrial crust movements, but that they one and all have been made by the long-continued excavating power of rivers and streams.

Such is the testimony of Prof. Spencer on these submerged valleys or fjords, which is distinctly opposed to Sir Henry's inter-

pretation of the history of these phenomena as sudden and cataclysmic. Sir Henry goes on to produce evidence of his postulated concurrent elevations of mountain chains, which, as he says, were very rapidly, if not suddenly, elevated at the same period, viz. the close of the Mammoth age.

Again, he observes:—"Just as I ventured to argue that the upheaval of the Ural Mountains, of the Altai, and of the American Cordillera was rapid, if not sudden, and marked by every sign of cataclysmic change, so also do I hold the corresponding subsidences to have been rapid, if not sudden." No ambiguity of expression here; Sir Henry seems to outdo his "old masters" in his appeal to catastrophism to explain his difficulties. He, however, naively says, "Evidence on such a point is not always easy to find"; just so, because the facts are all the other way. Mountain ranges were not formed in a day, but are the result of long-continued upward movements, of periods of repose, and also of depression.

It may be asked, what would be the effect of such sudden and violent terrestrial disturbances on the organic life of the globe? Undoubtedly great climatic changes would be induced, unfavourable and destructive to any unfortunate animal or plant that might be then existing; as no time would be given to the organism to adapt itself to its altered environment. Where are the signs and evidences of such destruction of life?

But to return to our mountain-uplifts—the very structure of mountain ranges, their various strata of different geological ages, their successive lines of cliff and other evidences of long erosion, all requiring time for their production, prove that their movements, as with subsidences, have, as a rule, been very slow and intermittent.

There was undoubtedly a very pronounced upward movement of a large area of North America at the close of the Tertiary period, to which American Geologists, with great show of probability, ascribe that severe climatic change which resulted in what is known as the "Glacial age"; but, if this continental uplift be the *cause* of the Glacial age, it does not square with Sir Henry's postulate as to the geological date of the changes of level which resulted, as Sir Henry assumes, in the destruction of the Mammoth at the *close* of that period.

It will be needless, I think, to pursue this subject farther, or to attempt to lighten "the burden of the parable which Sir Henry has been trying to preach" (I would rather say paradox). His recent pleadings in favour of his pet postulate of the "Mammoth and the Flood" appear to me as unreliable and unconvincing as those of his many previous contributions to the pages of the GEOLOGICAL MAGAZINE on the same subject.

VIII.—PROBLEMS CONNECTED WITH A COOLING EARTH.

By ARTHUR VAUGHAN, B.A., B.Sc.

IN the GEOLOGICAL MAGAZINE for June and July of this year, I stated a new theory to account for the inequalities of the earth's surface, and, at the same time, offered a criticism of Mr.

Reade's theory, as epitomised by him in the May Number of the same Journal. In a short reply, contributed by Mr. Reade to the September Number, he shows so clearly that he has totally misunderstood the physical reasoning upon which my theory is based, that some rejoinder is absolutely demanded.

In criticising my theory, he asks: "How then could the underlayers, by shrinking, exert such a huge pressure on the interior as to actually *compress the materials of the earth into a smaller volume*?" and, a few lines further on, he proposes that we should "work out *the decrease of volume* which would result from a given contraction of a shell of steel, 30 miles thick, acting on a sphere of the size and composition of our earth."

Did I rely upon actual decrease of volume due to pressure, my theory would, of course, be absurdly untenable; but my reasoning is based upon the *transference* of material from beneath a surface of great pressure to below a surface upon which the pressure is much less. This I have so frequently reiterated in my paper that I could quote from almost any page; perhaps the following extract, taken from my first paper, will suffice: "The underlying material will be, so to speak, squeezed out, and this will cause a real transfer from under the sinking area to beneath the surrounding regions."

To my criticism of his own theory Mr. Reade has not replied, but he has, however, objected to my attempt to show that the reasoning he employs is hardly sufficient to disprove the old contraction theory. Mr. Reade takes exception to a calculation, based upon his own figures, as to the elevation which could be produced from an outer shell too large, by a certain amount, for the interior sphere. I took a special case, and supposed the outer shell to be drawn out over the interior sphere, somewhat in the shape of a balloon, so as to produce over a certain large area a vast hollow cone-shaped elevation. I cannot understand how the very simple calculation necessary can be called in question, and I can only suppose that Mr. Reade has assumed that any elevation must be solid. If, however, we accept the theory of elevation by the folding due to lateral pressure, it is very difficult to see how all vacuities could be avoided, and yet the known dips of anticlines produced. It is, consequently, necessary in considering any elevation, which is not actually a maximum, to assume that elevation to be hollow.

The last point raised by Mr. Reade is one of much greater interest, and it directly introduces the main object of this paper. Stated in its simplest form, the physical problem is:—What would be the behaviour of a contracting shell closely fitted round a sphere unalterable in volume? (This is, in reality, the same as supposing such a shell to surround a less rapidly contracting interior.) Here we can easily distinguish two cases.

First, the shell may be contracting unequally at points whose distance from the centre is the same. This is the actual problem in the case of the earth, as I have shown in a former paper; and the results I believe to follow may be briefly recapitulated.

Any contracting area must attempt to become smaller, and this

object could be attained in two ways; either by splitting, that is, by overcoming the forces of cohesion between separate particles, or by moving bodily inwards, so as to become flatter and therefore smaller. The resistance to this last-named effect will arise from the pressure of the interior sphere, and in the case we are considering, of an unequally contracting shell, the effect will be to subject the interior sphere to pressure at all points, but to much greater pressure under the more rapidly contracting areas than under those whose contraction is less. The result seems clear, that the interior sphere will become deformed; material being transferred from under the areas of great pressure to beneath those of less, with the after effects I have stated in a former paper. The mechanism by which the solid interior sphere would thus become deformed can be illustrated by the consideration of two layers of closely touching spherical balls. If we suppose pressure to be applied over any area of the upper layer, the lower layer of balls beneath that area will be pushed asunder, and those which lie on the borders of the area of pressure will wedge themselves between the layers of balls surrounding the area, and thus produce a vertical elevation around the area of pressure.

Secondly, suppose an ideal case, that the shell contracts equally at all points equally distant from the centre. It follows that the pressure exerted upon the interior sphere will now be the same at all points of its surface, and, consequently, no transfer of material from areas of high to areas of low pressure can take place. The only relief to the forces of contraction can now be found in rupture or deformation.

Either of two effects may ensue. In the first place, the shell may, in contracting, split and each separate portion then contract normally, so that its contraction in any direction is linear. It is to be remarked that any want of homogeneity, or, in other words, any slight differences in contractibility, which may exist among the materials of which the shell is made, will aid in producing this effect. Secondly, the shell may stretch without splitting. This is a case of great interest as being the one usually assumed in problems dealing with the contraction of the earth's crust.

This assumption amounts to supposing any shell to contract by splitting, and then the separate portions to be pressed together again by the pressure of the overlying mass; much as if a very heavy roller had been pushed over their surface.

Now this second process will obviously evolve heat by the mutual rubbing of particle over particle; so that each shell must be regarded as an origin of heat, of which no account is taken in using the ordinary differential equation of conduction. The general effect will be to increase the time from first consolidation.

For, let us suppose the two processes to take place perfectly independently. First, let us suppose the shells to contract by splitting, so that no portion is in any way deformed, but decreases in volume, always remaining similar to itself. If, then, we further assume the conduction of heat to be unaffected by the vacuities so

formed, that is to say, if we suppose the heat to be uniformly conducted through the mass, and not to flow mainly along the separation surfaces (as is the case in the flow of electricity through a solid cylinder), we shall have the conditions which are assumed in the equation employed for solution.

Now let us imagine all the separate pieces of each shell rolled together; the further any shell is from the centre the greater will be the volume per unit area to be filled up, and, consequently, the greater the amount of heat evolved by viscous friction. Hence the difference between the temperatures of two consecutive shells, at any time, will be, in reality, less than that, had the first process alone come into play. Thus it would really take longer to establish any given temperature gradient; which amounts to saying that the time from consolidation would be increased. To make allowance for this new factor we should require some data as to the heat evolved by viscous friction in the rolling out of sheets of rock material.

Time from Consolidation.

Since physicists and geologists differ considerably in the number of years which they would allow for the world's age, it is a matter of great interest to see whether the numerical results arrived at by physicists should not be considerably increased.

I need scarcely remark that, in what follows, I am in no way concerned with the effect which any of the alterations suggested below may have upon the position of the theoretical level of no-strain; for I have already expressed my belief that the main elevations and depressions of the earth's surface have been caused by differential contraction.

In the determination of the time from first solidification, heat is supposed lost solely by conduction from within outwards, and no mechanical deformation or change of physical state is accounted for. Now, as I have shown above, heat must be generated in that settling together, which is considered to be the probable outcome of contraction, and this extra heat will, as I have shown, tend to increase the time from consolidation. So far there is a correction to be applied on account of the insufficiency of the equation employed to express all that actually takes place, for this equation takes account only of heat actually conducted from point to point, and does not recognise any heat originated within the cooling mass.

Again, the final result arrived at is, that the time varies as the square of the temperature of solidification directly, and as the coefficient of thermometric conductivity inversely. The temperature of solidification is assumed as 7000° F., and the coefficient of conductivity as 400. Both these quantities are supposed constant throughout the mass considered, and hence must be taken to have their average values throughout that mass.

But when the Earth was molten, the pressure at any point within a few hundred miles of the surface, was practically that due to the weight of the overlying mass; hence it follows that the molten

rock would solidify at a very much higher temperature than at the surface. The average value assumed must represent the temperature at a great depth; in fact, the constant multiplier of the integral, which occurs in the equation connecting temperature, depth, and time, is found by assuming that the temperature of solidification is the average value which it has at a very great depth. It is, then, quite beside the question to employ the temperature of solidification of slag or other molten rock at the surface, for we really require to know at what temperature the rock would solidify under a pressure of, say, a hundred miles of rock. At the surface molten slag solidifies at about 3000° F.; it seems then, that the assumption of 7000° F. for use in the problem under consideration, leans very much on the side of under-estimation.

Again, the conductivity or, in other words, the number which represents the amount of heat which escapes through a given thickness of rock in a given time is assumed as 400°, a number obtained by experiments on rocks at the surface. But the greater the temperature, the greater must be the instability of the mean centre of oscillation for each molecule, and this instability must increase until the fusion point is reached, when the molecules have free motion over each other. Hence, the hotter the rock, the more heat will be absorbed in molecular action, and, consequently, the less will be the heat conducted through in any given time; or, in other words, it seems probable that the conductivity must diminish as we reach a greater depth beneath the surface. It follows, that the average value of the conductivity must be much less than its surface value. But any decrease in this value increases the time in the same ratio. A further cause acting in the direction of increasing the time has already been pointed out, though probably of no great weight and not worthy of consideration, beside the corrections suggested above. This consists in the conduction being diminished when heat has to traverse rocks stratified or laminated transversely to the flow of heat. Since the rocks near the surface have their separation planes, as a general rule, inclined at a low angle, the stratified rocks of the crust must conserve the heat passing through by conduction; though this will be, to a small extent, counteracted by the existence of vertical joints which allow a more rapid loss by a kind of surface creep.

In fine, the geologist and biologist have a right to demand of the physicist first, that all losses of heat, other than those considered in his calculations, should be shown to have a negligible effect on the result; and, secondly, that the numerical data he assumes should be at least greater than the probability, not less. Results obtained by the substitution of values, formed on considerations of phenomena taking place under atmospheric pressure, and, as in the case of the determination of conductivity, at low temperatures, can lead to nothing but misconception. There seems no reason why the estimate of 100 million years should not be increased to 1000 million, which would almost allow as much time as the evolutionist desires.

NOTICES OF MEMOIRS.

Abstracts of Papers read before the British Association at Oxford, August 9–14, 1894.

I.—On a Keuper Sandstone cemented by Barium Sulphate from the Peakstones Rock, Alton, Staffordshire. By W. W. Watts, M.A., F.G.S.

PROFESSOR F. CLOWES[1] has described a sandstone from the Himlack Stone, near Nottingham, in which the grains were cemented with crystalline barytes, the amount of this material varying from 28 to 50 per cent. in different specimens. This rock occurred at the base of the Keuper sandstone of that locality. A somewhat similar rock, occurring at about the same horizon, is described by Mr. A. Strahan,[2] from Beeston Castle in Cheshire, and the same author refers to the frequent occurrence of barytes in the Keuper breccias.

Bearing these facts in mind, the writer visited a curious isolated stack of rock, called the "Peakstones Rock," near the village of Alton in Staffordshire, which is figured in Professor Hull's Memoir on "The Triassic and Permian Rocks of the Midland Counties of England." This stack is made of the Lower beds of Keuper sandstone, but its outer portion has lost whatever cement it may once have contained. It is, however, situated at the end of a spur which projects into a valley, and exposes a good deal of bare rock. This rock contains what at first look like several veins of barytes two or three inches thick, striking along the spur and straight through the place occupied by the Peakstones Rock. On examination of specimens the veins are seen to be planes in the sandstone cemented by barytes. The specific gravity of the rock is 3·09, and, as the grains are chiefly subangular fragments of quartz and felspar, it must contain about 28 per cent. of barytes. This almost insoluble cement has undoubtedly given rise to the spur above alluded to, and almost as certainly has caused the survival of the Peakstones Rock. This is, however, so much exposed to the weather on all sides, and both to mechanical and chemical disintegration, that if any cement is still left it must be in the inner part of the mass, which cannot be reached by ordinary means. Another specimen from west of Kent Green, near Congleton, containing barytes, and with a structure very like that described by Mr. Strahan, was also referred to.

II.—Sporadic Glaciation in the Harlech Mountains. By the Rev. J. F. Blake, M.A., F.G.S.

THE author drew a distinction between two results of glaciation—the one, negative, in which the rocks are rounded and striated, and all or nearly all the débris removed; the other, positive, in which the rocks are covered by a thick deposit of drift with

[1] Rep. Brit. Assoc. 1885, p. 1038; 1889, p. 594; 1893, p. 732; and Proc. Roy. Soc. vol. xlvi. pp. 363–369.

[2] Mem. Geol. Survey. Exp. Quarter Sheet, 80, S.W. p. 7.

boulders. In the Harlech Mountains district areas showing these opposite results lie side by side. Most of the glaciation is of the negative kind, but the areas drained by the Crawewellt and the Ysgethin are covered by glacial cones of dejection. This difference is accounted for in the first instance by the local drainage being opposite to the general drainage, and in the second by the small size of the gathering ground for the ice. From these results it was argued: 1. That drift deposits are, as a rule, left beyond the area of ice-flow. 2. That no submergence could possibly have taken place here since the Glacial period, or the features above noted would have been obliterated.

III.—On the Mechanics of an Ice-Sheet.—By the Rev. J. F. Blake, M.A., F.G.S.

THE author attempted to explain how an ice-sheet can carry boulders up a slope, and leave them at a height of 1,000 feet or more above sea-level. The sides of the channel are, in the first instance, supposed to be parallel, so that the mass of ice may be represented in a diagram by its longitudinal section. Taking, for simplicity, the shape of the surface moved over to be represented by two straight lines, one corresponding to the slope down from the mountains, the other the slope up from the sea-bottom to the final destination of the boulders, and, taking the surface of the ice as flat, the ice-sheet is represented by a triangle. This is supposed to settle down in such a way that, though the level of the end is higher, the centre of gravity of the whole is lower. This fall of the centre of gravity is the effective cause of the motion of the ice-sheet, the resistance to be overcome being that of the ice to change its shape. If the ice-sheet be supposed divided into strips parallel to the slope from the mountains, these will be like a series of overlapping glaciers, and under the influence of the pressure will swell out at the bottom, and thus push the further end of the whole mass a little way up the counter-slope. Continual additions of snow at the end where the ice-sheet commences, or elsewhere on its surface, will be cumulative in their effects, and thus the further end of the ice-sheet will ultimately ascend as required. Again divide the triangle into strips by lines parallel to the counter-slope. The lower of these strips will be pressed together, and any point on the base will be carried on in the direction of the whole motion at a greater rate than the higher layers, and thus the stones, etc., on the sea-bottom will be pushed up to their final resting-place, and anomalies of distribution might thus be accounted for by the previous dispersal of the boulders. It was then shown that differences in shape of the ice-sheet and its spreading out at the further end will make little difference in the argument, and under certain conditions will aid the motion.

The author then discussed the question of the glacial erosion of lakelets, and indicated the conditions under which this is possible, particularly referring to the difference between an ice-sheet such as that dealt with in the paper and an ordinary glacier.

IV.—On the Deposit of Iron Ore in the Boring at Shakespeare Cliff, Dover. By Professor Boyd-Dawkins, F.R.S.

THE general results of the boring at Dover were laid before the British Association at Cardiff in 1892, so far as relates to the discovery of the South-eastern Coal-field. In the present communication the author treats of a bed of ironstone, which is likely to be of great importance in the new industries which will spring up sooner or later in Kent in consequence of the discovery of Coal in workable quantities.

The strata penetrated in the boring are as follows:—

		Feet.
Upper Cretaceous	Lower Gray Chalk and Chalk marl	130
	Glauconitic marl	8
	Gault	121
Neocomian	Folkestone Beds	64
	Sandgate Beds	77
	Hythe Beds	87
	Atherfield Clays	18
Oolitic	Portlandian	32
	Kimmeridgian	73
	Corallian	159
	Oxfordian, Callovian	188
	Bathonian	156
Coal-measures with twelve seams of Coal 23 feet 5 inches thick		1068½

The ironstone occurs in the Kimmeridgian part of the section, and as shown in the following details:—

Portlandian Beds:—	Feet.
Gray marl with oolitic grains of ferric oxide	2
Hard gray limestone	1
Brown calcareous sandstone	2
Gray shelly limestone with oolitic grains of ferric oxide	1
Dark-gray marl	2
Hard blue limestone with *Littorina*	1
Brown oolitic ironstone	12
Gray limestone	4
Dark bituminous clay	8
Flaggy sandstone	2
Gray sandy clay	4
Arenaceous limestone with *Cidaris*	7
Dark bituminous shale	27
Gray nodular limestone	2
Coralline Oolite with the usual fossils, *Pecten vagans*, etc.	27

The ironstone presents very singular physical characters. It is composed of small dark-brown shining grains of hydrated oxide of iron like millet seed, embedded in a crystalline base partly of calcium carbonate, and partly of iron carbonate. These grains are oolitic in structure, and are probably the result of the same chemical change by which the calcareous beds of the Inferior Oolite in Lincolnshire have been converted into iron ores. They occur, it will be noted, in several strata above the main bed, 12 feet in thickness in the above section.

This bed of iron ore is identical with that described by Blake and Hudleston at Abbotsbury in Dorset, where it occurs between the Kimmeridge clay above and the Corallian rocks below.

It is also physically identical with the valuable iron ore worked for many years at Westbury in Wiltshire, where it is met with at a lower horizon, being there separated from the Corallian limestones by 4 feet of marls and sands.

This stratum, although probably of purely local origin, is to be looked for in the beds above the Corallian throughout the whole of Southern England, from Dorset eastwards. Its discovery at Dover is only second in importance to that of the South-eastern Coal-field. It will have to be taken into account in the future development of the Coal-fields in Southern England.

V.—The Volcanic Phenomena of Vesuvius and its Neighbourhood. Report of the Committee, consisting of Mr. H. Bauerman, Mr. F. W. Rudler, Mr. J. J. H. Teall, and Prof. H. J. Johnston-Lavis. (Drawn up by Prof. H. J. Johnston-Lavis.)

SINCE the last report lava has continued to pour forth from the top of the new lava-cone in the Atrio del Cavallo, sometimes in small quantities, at others in considerable abundance. On no occasion, however, did the lava issue beyond the limits that it had reached in the years 1891–92. In fact, the whole of that eastern part of the Atrio known as the Val d'Inferno has not been invaded at all by the new lava during or since its issue in the spring of 1891. The consequence of this has been that it has continued to pile itself up around the line of fissure by which it issued, and still further add to the dimensions of the great lava-cone that it had built up in the Atrio. So great has this cone become that it constitutes a prominent feature in the outline of the volcano as seen from Naples. The eminence of Somma is separated from Vesuvius by the depression of the Atrio. This notch, so to speak, in the general outline was terminated below by an almost horizontal line, which is now replaced by an obtuse cone, so that many people speak of three summits to the Vesuvian volcano. This is rather an exaggeration, for although the new lava-cone is of very considerable dimensions, for the time occupied in its growth, yet it cannot compare with that of the cone of Vesuvius on one side or the ridge of Somma on the other.

The whole of this new cone is entirely built up of lava, by far the greater part being of the *pahoehoe* or corded type; only now and then, during marked activity, has there been produced any lava with a rugged scoriaceous surface. The occasion was therefore a very valuable one to determine the slope of such a lava-cone. This was done only normally to the line of fissure by which the lava issued, and which makes the cone terminate in an elongated ridge rather than in a point. Practically all these clinometric observations, which were taken with great care, gave angles varying from 13° to 15°.

Comparing this angle with that of such mountains as Etna or Mauna Loa we must consider that both are composite cones, and have experienced many disturbing influences, such as the formation of parasitic eruptive outlets, from which lava streams have issued far

away from the summit, and have thus diminished the general slope of the volcano. Those mountains are usually considered to have an average slope of 10°. The Hawaiian lavas are, as is well known, exceptionally fluid, and we could hardly expect cones of greater slope than 10°. At Etna the lavas have always been more viscous from their lower temperature and the compound or false viscosity given to them by the large number of porphyritic crystals already existing in the magma at the time of emission, just as earth mixed with water may produce a viscous mud. These new lavas of Vesuvius, as is the case with all those that issue high up on the volcano and in small quantities, were very viscous owing to their low temperature and advanced crystallisation, so that soon after the material poured out it was prevented from flowing by slight further cooling. We may take, therefore, this average slope of 14° as the best and most correct estimate for a lava of this nature.

This recent outflow exhibits most of the varieties of surface to be met with in the type of lava above mentioned, such as corded shapes of different kinds, irregular globular surfaces, sheets, and plates either in position or reared on end, and tunnels of every variety, frequently with continuations as walled canals. A magnificent lava hump was formed right under the escarpment of Somma. The origin of these humps is still obscure. They are common on most large flows of corded lava of Vesuvius, but, unfortunately, I have never been present at their formation, nor do I know of anyone who has.

The points of issue of the lava occurred at various spots along a line corresponding with the strike of the radial dyke to which it owes its origin, so that the new lava has as a summit an irregular ridge running nearly north and south. Of course the actual highest point is nearly always that where the last lava issued. Generally more than one spot along this line gave out lava at the same time. The fluid rock flowed sometimes on one side, sometimes on the other, so that the general public at Naples were only from time to time treated to a glimpse of Nature's fireworks, and when the lava flowed in the opposite direction it was often announced that it had altogether stopped.

During the last year several new conical spiracles were formed, but none of them comparable in perfection of form to those described in the last two reports, or exhibiting equally interesting features.

No very interesting minerals were produced as sublimates. In fact, only two species are worthy of mention. On one occasion a small quantity of tenorite was formed in one of the spiracles. Soon after the lava had entirely stopped flowing in February, sublimates of potash-bearing halite were very abundant around the vents, in beautiful fern-like skeletons, in which a number of feathery branches radiated at right angles from a stem representing usually about three edges of a cube, and were themselves so many edges of smaller cubes. Sometimes this halite was gray, from minute hæmatite crystals being deposited with the salt, which likewise was in some cases greenish from copper impurities. Most, however, were of a beautiful snow white. One small cavity in particular, about the

size of a man's body, was clothed with the most glistening white lining, and from the roof and walls showers of crystals fell from time to time. These were not visibly red-hot in bright diffused daylight, but looking towards the shaded inner extremity of the cavity, a bright red incandescence was visible. In a short time, with suitable apparatus, I collected over two kilogrammes of this material absolutely free from mechanical impurities.

Along many of the cracks of the lava beautiful glassy crusts of halite, more or less impure, were formed, and often showed a dull red heat in daylight. These crusts on being removed become rapidly opaque and milky in hue, and audibly cracked into starch-like columns, due to the rapid contraction on cooling—producing, in fact, a miniature basaltic structure.

About February 5th, 1894, the lava was issuing in very small quantity, and by the 7th showed no trace of movement. Yet even in May cracks in the lava near its point of exit were incandescent some distance in, and the saline incrustations mentioned above were in full perfection.

Coincident with the arrest of the lateral outflow, the lava rose in the chimney, and the red reflection from the top of Vesuvius that had been absent for so long, with rare exceptions, was again almost daily visible. The level of the lava in the main chimney soon rose to the bottom of the new crater that had been forming, and increasing in size during the time the lateral issue of lava had been going on, and commenced the filling up of that cavity by the formation of a cone of eruption, so that almost coincident with the arrest of the leakage of lava laterally the central activity changed from the crater- and dust-forming stage to the lava cake- and cone-forming stage.

I made a careful examination of the summit of Vesuvius about the middle of May. The crater in an east and west direction was about 150 m. in diameter, and its depth, then decreasing, was about the same. The walls were remarkably steep, in some places even vertical or overhanging. The bottom could be seen with difficulty owing to the crumbling nature of the edges. The walls are nearly all covered by sublimates or dust that has adhered and crusted them over, so that several dykes, both solid and hollow, can no longer be distinguished. This is especially the case with the one formed during the 1891 outburst. The details of the great rift of the 1880–81 and subsequent eruptions on the east side of the great cone were still easily discernible. On the south side, and a little to the east, a wall of rock stands out from the side of the crater and is directed nearly towards the centre. It is capped by a pinnacle of rock, and is really the old dyke of the 1885 eruption.

Just to the east of that wall, and partly owing to its existence, the slope of the inside of the crater is less in that direction. Here the guides had made a little path for a few metres down. On examining carefully the condition of things from its lower termination, which so far aided little the view of what was going on at the crater-bottom, I found that by extending it down a slope, and then cutting a ledge farther round to the east at a suitable point, a bracket-like

platform some metres square could be reached, which is about half way down the crater. Later the path was further widened by me and made more commodious, and now gives easy access to the platform, from which one can look right into the vent of the volcano and watch with ease the boiling up of the lava and the ejection of the great blobs and cakes that are rapidly filling up the crater. Unfortunately, owing to the well-like shape of the crater, the shadows due to the vapour column spreading out overhead, and the dark colour of the rocks, instantaneous photography could not be utilised to record this interesting and everchanging scene.

As is usual at some period after an eruption, feathery gypsum is a common product in the cavities of the old scoriæ, and is associated at the fumaroles with a little sulphur (an exceedingly rare mineral at Vesuvius), with abundance of molysite and kermesite.

In the Campi Phlegræi little of novelty has come to light. A tunnel and a deep shaft which is being constructed in Naples to complete the drainage works have brought several interesting sections to light, but not of sufficient completeness to be yet worth recording.

VI.—On Certain Volcanic Subsidences in the North of Iceland.

By Tempest Anderson, M.D., B.Sc., F.G.S.

PERHAPS the most striking features in Icelandic scenery are the *giás* (pronounced "geow"), or fissures and chasms, which are so frequently met with in all the districts in which recent volcanic activity manifests itself. They are usually, and in most cases rightly, ascribed to the lower stratum of a molten lava stream, having obtained an outlet after the surface has consolidated into a crust of greater or less thickness.

Giás of this class are, so far as the author has been able to observe, confined within the limits of a single lava stream, and do not affect previously formed rocks. Usually there is a large giá roughly parallel with each side of the original lava stream, and the space between these has subsided considerably. Any giás in this subsided portion are much smaller, and obviously of secondary importance. Examples of this are to be found in the well-known Almanagiá, at Thingvalla, which has a throw of about 100 feet, while the sides of the smaller giás which enclose the Logberg in the subsided portion are practically on the same level.

There are also several such subsidences near Lón and Ásbergi, in the north of Iceland. The main subsidence at Ásbergi is a little more complicated, though evidently due to the same causes. Here a large roughly triangular area has subsided, the throw at the apex being probably nearly 300 feet, but a space in the middle has remained at its original height, so that a depression has been produced like a great V, the portions both between and outside the legs having remained standing. In the case of Thingvalla it appears not unlikely that the lava which flowed down into the lake solidified on coming in contact with the water, and formed a wall sufficiently strong to hold up the lava plain till it formed a firm crust, and that

the giving way of this and the escape of the molten lower layers into the deeper parts of the lake caused the subsidence.

Similarly the lava which escaped from Ásbergi may have been that which now occupies the low ground near the estuary of the Jokulsá, in the direction of Lón.

On the east and south-east of Lake Myvatu a very extensive eruption, or series of eruptions, has taken place from a chain of craters locally called Gardr Borgir ("the castles of Gardr," which is the name of a farm). The lava flow has occupied nearly all the bed of Lake Myvatu, and flowed down the valley of the Laxá to its mouth at Laxamyri. All this stream of lava is very remarkable for the number and size of the spiracles with which it is studded, and a regular gradation of sizes exists, between spiracles the size of a haycock and cones some of which cannot be less than 200 feet high. These cones and craters, which constitute such a striking feature of Lake Myvatu, may probably be nothing more than spiracles formed by the escape of steam generated by the conflict of the hot lava with the water of the lake. The barrier which holds up the water of the present lake consists of this lava, and caves exist in it which are obviously channels by which molten lava has escaped. These and deeper-seated ones would be those by which the lava escaped and left the depression occupied by the present lake. Between the craters of eruption and the lake no spiracles were noticed, but there is a very remarkable series of rocks—the Dimmuborgir—masses of lava of fantastic shape, 30 or 40 feet high, which have remained standing while the intervening portions have subsided. They present slickenside marks where the subsiding portions have scratched the masses that have remained standing, and tide-marks where the crust has halted in its descent; also in many places bulgings, where the lava has been scarcely stiff enough to stand, and others where it has actually formed stalactitic masses.

So much for actual lava subsidences.

The special object of this paper is to draw attention to a subsidence on the slopes of Leirnukr, a volcano several miles north of this spot, where a large strip of land, perhaps 200 yards wide, and one mile or more long, has been let down to a varying depth, averaging perhaps 60 to 80 feet.

The faults bounding it, like nearly all the fissures in this district, run north and south; and the east face, which is most perfect, cuts right through a thick stream of old columnar lava and through a large boss of tuff, round and over which the lava has bedded itself, and also through the tuff rocks at each side of the lava stream. It would appear worthy of consideration whether this great depression, which thus affects all the crust of the volcano impartially, may not have been caused by the falling in of one of the steam cavities which may be presumed to exist under volcanoes after the lavas have been expelled by the steam pressure.

This would accord with the observation that sedimentary rocks near volcanoes often dip towards those volcanoes. Mr. Goodchild has informed the author that the sedimentary rocks round Arthur's

Seat are much thicker the nearer they are to that old volcano, as the ground had slowly sunk while they were being deposited.

Near Lón the author was shown a small giá, said to have b formed during an earthquake in February, 1885. The crack v of a freshness corresponding to such a date, and was only a f inches wide, and so short that it could not be determined whethe extended beyond one bed of lava. It certainly was not an exam of the escape of liquid lava from below a crust, nor of a subside over a steam cavity, and its chief interest in this connection is showing that at least three separate sets of causes are at work producing the giás of Iceland.

VII.—On a New Method of Measuring Crystals, and its App cation to the Measurement of the Octahedron Angle Potash Alum and Ammonia Alum. By H. A. Miers, M. F.G.S.

THE two fundamental laws of Crystallography—namely (1) t constancy of the angle in crystals of the same substance, a (2) the law of simple rational indices—seem to be violated by th crystals which are liable to irregular variations in their angles, those which have the simple faces replaced by complicated "vicina planes.

Both these anomalies are exhibited by potash and ammonia alu Brilliant and apparently perfect octahedra of these salts show lar variations in the octahedron angle; other crystals show low vici planes in place of the octahedron faces.

If it be true, as is supposed, that the octahedron angle varies different crystals, it would be interesting to ascertain whether p gressive variations can be traced during the growth of a sing crystal, and whether some or all of the octahedron faces change th direction in space if the crystal be held fixed during growth.

In order to solve this problem a new goniometer has been co structed, in which the crystal is fixed at the lower end of a verti axis, so that it can be immersed in a liquid during measurement.

This device is in reality an inversion of the ordinary goniomet with horizontal disc; the liquid is contained in a rectangular gl trough with parallel-plate sides; one side is placed rigidly p pendicular to the fixed collimator, and the other is perpendicular the telescope, which is set at 90° to the collimator. The trough supported on a table which can be raised and lowered, so that t crystal can be placed at any required depth in the liquid. If t liquid used be its own concentrated solution the crystal can measured during growth, and the changes of angle, if any, can observed at different stages.

In order that it may be held rigidly, the crystal is mounted, wh small, in a platinum clip, which it envelops as it grows larger.

The results derived from the measurement of a large number alum crystals are as follow:—

(1) The faces of the regular octahedron are never developed up alum growing from aqueous solution.

(2) The reflecting planes (which are often very perfect) are those of a very flat triangular pyramid (triakis octahedron), which overlies each octahedron face.

(3) The three faces of this triangular pyramid may be very unequal in size.

(4) The triakis octahedron which replaces one octahedron face may be different from that which replaces another octahedron face upon the same crystal.

(5) During the growth of the crystal the reflecting planes change their mutual inclinations; the triakis octahedron becomes in general more acute, *i.e.* deviates further from the octahedron which it replaces as the crystal grows.

(6) This change takes place not continuously, but *per saltum*, each reflecting plane becoming replaced by another which is inclined at a small angle (generally about three minutes) to it.

(7) During growth the faces are always those of triakis octahedra; if, owing to rise of temperature, re-solution begins to take place, faces of icositetrahedra are developed.

Conclusions:—The above observations prove that the growth of an alum crystal expresses an ever-changing condition of equilibrium between the crystal and the mother liquor. It does not take place by the deposition of parallel-plane layers; new faces are constantly developed: since these succeed one another *per saltum* they doubtless obey the law of rational indices, though not that of *simple* rational indices.

From the mutual inclinations of these vicinal faces it is possible to calculate with absolute accuracy the angle of the faces to which they symmetrically approximate. This angle is found to be that of the regular octahedron, $70° 31\frac{3}{4}'$. The octahedron angle of alum is not, therefore, as appeared from the observations of Pfaff and Brauns, subject to any variation.

The angle at which a given vicinal plane is inclined to the octahedron is independent of the area of the plane, and of the temperature of the solution, and of the barometric pressure: it appears to be conditioned by the concentration of the solution at the surface of the plane.

In confirmation of this view it is found that the upper and lower portions of an octahedron face which stands vertical are often replaced by two different triangular pyramids; also that the three faces of one such pyramid are, at a given moment, not necessarily equally inclined to the octahedron face which it replaces.

When, as is often the case, one of the three vicinal planes is large, and the other two are too small to give a visible reflection, the face appears to be a single reflecting plane. It is this which has been mistaken for the octahedron face in previous observations.

Similar phenomena of growth are exhibited by crystals of other substances belonging to different systems. The conditions of equilibrium between the crystal and the solution are such that vicinal planes appear in place of simple forms; these vary with the concentration of the solution, and give rise to variations in the

measured angles, which are only apparently anomalous. Their true position can be determined on a crystal of cubic symmetry (such as alum) whose theoretical angles are known.

A further study of the faces developed during the growth of crystals will, it is hoped, lead to a better understanding of the reasons why a simple face like the octahedron should not be a surface of equilibrium, and of the relation between the vicinal planes and the structure of the crystal.

REVIEWS.

I.—Memoirs of the Geological Survey of the United Kingdom. The Jurassic Rocks of Britain. Vol. IV. The Lower Oolitic Rocks of England (Yorkshire excepted). By Horace B. Woodward, F.G.S. 8vo. pp. xiv. and 628, with 2 Plates and 137 Woodcuts. (London: Kegan Paul, Trench, Trübner & Co., Limited, 1894.) Price 10*s*.

THIS is, practically, a third instalment of the important work now in course of publication by the Geological Survey, of which the previous volumes have already been noticed in the Geological Magazine. Vol. iv. contains an account of the Lower Oolites (Inferior Oolite and Great Oolite) throughout their long outcrop from the English Channel to the Humber.

In his preface Sir A. Geikie observes that much assistance has been derived by Mr. Woodward in the preparation of the present volume from the previous Memoirs of the Survey, especially those dealing with the Oolitic rocks by Professors Hull, Green, and Judd, and more recently by Messrs. Ussher and Jukes-Browne. The Director-General also does justice to the work of other observers in this field, commencing with William Smith, whose original labours are commemorated in the names given by him to many of the subdivisions of the Oolites.

The author, in fact, has largely availed himself of the assistance to be derived from previous publications, both official and non-official; and he likewise acknowledges the help which he has from time to time received from the personal communications of those interested in the Jurassic geology of this country. It is of course obvious that with such a work as Judd's Geology of Rutland in hand, the path of the surveyor in the East Midlands must have been made comparatively easy.

After some introductory remarks by Mr. Woodward, dealing with the Oolitic rocks as a whole, we have some petrological notes from Mr. Teall; nor is the subject of *Girvanella*-tubes forgotten in connection with the possible origin of Pisolite. Two plates of sections of Oolitic limestones and ironstones illustrate this portion of the work.

The Inferior Oolite Series (*Bajocian*). Chapters ii. to vii. inclusive, are devoted to this series, which, in its course throughout England, is justly described as exhibiting almost every variety of stratified rock. The base, Mr. Woodward considers, is not always

well-defined. From Dorsetshire to the Vale of Moreton there occurs a group of sandy passage-beds which he thinks are most conveniently designated by the term "Midford Sand," comprising the zones of *Am. opalinus* and *Am. Jurensis*, between which the division of Lias and Oolites is taken. The discussions on these points between the author on one side and Messrs. Buckman and Hudleston on the other, are familiar to the readers of the Quart. Journ. Geol. Society. East of the Vale of Moreton the base of the Inferior Oolite is less connected by passage-beds with the Lias, and, as we pass across Central England into Lincolnshire, the Northampton Sand and its equivalents form a base which is clearly separated from the Upper Lias. Such also, we would observe, appears to be the case generally in Yorkshire, except at Blue Wyke, where these same passage-beds, more perfectly developed, perhaps, than elsewhere in England, are seen to connect the Upper Lias with the Dogger. We must not forget, either, that even in the South-West of England, where there is an overlap of the *Parkinsoni*-zone, as is frequently the case, no passage-beds intervene between the Ragstones and the beds on which they rest.

In the south-west also the upper limits of the Inferior Oolite are more easily defined, as the contrast between its uppermost limestones and the Lower Fuller's Earth is strong, and the line readily drawn. But when we reach North Oxfordshire, without any typical Fuller's Earth as a guide, whilst the Inferior Oolite itself is changing and putting on higher beds, often of an extremely sandy character, the upper limit has not always been easy to define. The result has been an indefinite mapping of the area. Thus Mr. Woodward writes (p. 148): "It is clear that the mass of the sandy strata previously grouped as belonging partly to the Great Oolite and partly to the Inferior Oolite belongs to the latter series. . . . . There is no more complicated tract among the Oolitic rocks of England than this region of the Inferior Oolite between Chipping Norton, Charlbury, and Banbury. We enter a region of changing sedimentation, which to some extent corresponds with the change in the general strike of the beds." Further eastwards the upper limits are not always clear in the absence of the Lincolnshire Limestone; although, in section, the Lower Estuarine may generally be distinguished from the Upper Estuarine.

The zonal divisions and varying developments of the Inferior Oolite are points of great consequence, and it may also be added of great interest, to the student of Jurassic geology. Hence their differentiation and correlation constitute by no means the least important part of this work. For convenience of description Mr. Woodward follows a grouping already indicated by Mr. Hudleston, and refers to the fossils of the Inferior Oolite under the following principal zones:—

Upper Division	*Am. Parkinsoni.*	Lower Division	*Am. Murchisonæ.*
	Am. Humphriesianus.		*Am. opalinus.*

These zones, as is well known, are best developed in the Dorset area, where an essentially Cephalopod facies prevails; and in that

district it is possible to subdivide them, as Mr. Buckman has lately done, into sub-zones or hemeræ. In the far more bulky deposits of the Cotteswolds such precision is scarcely possible; whilst in the Inferior Oolite of Lincolnshire, where as a rule a thoroughly non-Cephalopod facies prevails, any subdivisions which may be attempted in the limestones must be based on the general character of the fauna rather than on Ammonites which are seldom in evidence.

The varying development of the Inferior Oolite, in its local details, is well worked out in Chapters iii. to vii., as this protean formation is followed from the thin but highly fossiliferous ironshot oolites of Dorset and part of Somerset, through the thick false-bedded oolites and ragstones of the Cotteswolds, into the sandy limestones of North Oxfordshire. In this region the Inferior Oolite begins to suffer an eclipse even on the outcrop, whilst on the dip generally we know that it thins out to nothing beneath the overlying Bathonian, the highest beds in most cases being widely transgressive over all the others. The typical developments in the counties of Northampton and Lincoln, which belong to the northern or Yorkshire province of the Inferior Oolite, are partly foreshadowed in North Oxfordshire, as, for instance, at Otley Hill, where, beneath 3 feet of Ragstones representing the Upper Division, occur 4 feet of hard, sandy and ferruginous limestones which are regarded as in part the equivalents of the Northampton Beds.

The Northampton Sand itself, "where fully developed, represents the upper part of the zone of *Am. Jurensis*, the zone of *Am. opalinus*, and portions of the zone of *Am. Murchisonæ*." The exact horizon of the overlying Lower Estuarine Beds is not quite clear. Judging from the fauna there is good reason to suppose that the lower and larger portion of the Lincolnshire Limestone is on or about the horizon of the Oolite Marl. It is probable, however, that higher beds succeed, and we cannot agree with the author in his suggestion that the Great Ponton Beds may be merely a repetition of these in the Little Ponton cutting. A study of the fauna is clearly in favour of the view that the Great Ponton Beds are high in the series, many of the fossils being Bathonian in character, whilst in the more northern cutting some of the most characteristic fossils of the Oolite Marl may be noted. As we proceed towards the Humber the representative of the Northampton Sand has had the Yorkshire term "Dogger" applied to it, whilst the Lincolnshire Limestone has been divided into an upper series (the Hibaldstow beds) and a lower series (the Kirton Beds), though, so far as is known at present, there is no marked palæontological distinction.

The Great Oolite Series (*Bathonian*) presents a marked contrast to the preceding one in the comparative constancy of its Ammonite species, which, moreover, are by no means numerous. The following grouping is adopted.

Cornbrash. Zone of *Am. macrocephalus*.

Zone of *Am. arbustigerus*	Forest Marble and Bradford Clay	Great Oolite Clay
	Great Oolite and	Great Oolite Limestone
	Stonesfield Slate	Upper Estuarine Series
	Fuller's Earth	

Under this arrangement the Fuller's Earth, or "Fullonian," is regarded as a member of the Great Oolite Series, and, indeed, Mr. Woodward considers that its upper portion is intimately connected with the Stonesfield Slate where that phase of the Great Oolite is developed. He also confirms Mr. Etheridge's numerical estimate as to the preponderance of Bathonian species throughout the Fuller's Earth.

None of the subdivisions of the Great Oolite can be regarded as being constant, and excepting in the Stonesfield Slate its Lower Division contains few, if any, distinctive fossils. Practically there is no zonal grouping, and the fossils for the most part range throughout. Lycett, it will be remembered, drew attention to this circumstance, and was thus disposed to minimize the value of the Great Oolite subdivisions. Indeed, he went so far as to suggest that the Forest Marble and Bradford Clay might be omitted altogether without detriment to science. Doubtless, as marking time in a palæontological sense, there is much truth in this, and we have always thought that far too much importance was attached to these subdivisions in the south-west of England. However, as local developments, they are worthy of note by a Geological Surveyor, and all the more since Mr. Woodward has shown that the Great Oolite of Bath and Bradford does not pass into the Forest Marble of Dorset. As originally held by William Smith, there seems to be evidence in part of the Dorset area of the mass of the Great Oolite having been eroded. This, at any rate, is Mr. Woodward's view, though it is just possible that the Great Oolite Limestone may have originally been a lenticular deposit, such as the Lincolnshire Limestone was described by Judd and Sharp.

On the whole Mr. Woodward concludes that the Forest Marble or Bradfordian has some claims to be regarded as a subdivision of the Great Oolite Series altogether independent of the Great Oolite itself. And yet he (pp. 271, 272) elsewhere speaks of the difficulty of separating the Forest Marble and Great Oolite, there being no band that can be relied upon as a constant horizon in the series. This uncertainty is exhibited in the difference of opinion as to the position of the "Kemble Beds," which were mapped by Prof. Hull as part of the Forest Marble, though Mr. Woodward endorses the view of the late Prof. Buckman that they should be regarded as a portion of the Great Oolite. Thus, the general section of the Great Oolite (by Prof. Buckman) adopted for the Cotteswold district is as follows:—

Upper Division.	Ft.	in.
Yellowish Oolite [=Kemble Beds]	45	0
Marl, etc., with *Lima cardiiformis* and *Terebratula maxillata*	4	0
Hard Limestone with *Purpuroidea*, *Pachyrisma*, etc.	6	0
White Limestone	30	0
Lower Division.		
Rough Freestone or Ragstone	25	0
Stonesfield Slate	15	0
	125	0

The Kemble Beds are not seen in the Minchinhampton Quarries, the highest beds there visible he regards as belonging to the "White Limestone."

Although palæontological evidence is not a certain guide for fixing horizons in the Great Oolite, it is probable that in North Oxfordshire the Great Oolite Series, as it is observed to rest on the Chipping Norton Limestone, does not present precisely the same basement beds throughout. About Chipping Norton itself there seems to be a definite line and a thorough physical break at the base of the Great Oolite, and this is further evidenced by the occurrence of the "rift bed," where a clay with Bathonian fossils fills up cracks in the limestone of Inferior Oolite age. In this district also the equivalents of the Stonesfield Slate may be observed to occur, as, for instance, in the famous cutting at Langton Bridge. This section seems to corroborate Mr. Walford's recent discoveries at Stonesfield, which go to prove that the Stonesfield Slate is not the actual base of the Great Oolite in that area, as some sort of limestone, with clay seams, presenting on the whole the aspect of the Great Oolite, is found to occur beneath the slate bed.

Reverting once more to the Forest Marble, with its locally developed Bradford Clay, the author considers that, in spite of the varying nature of its particular character, when taken as a whole, it forms a fairly well-marked division, extending from the coast of Dorset to the neighbourhood of Buckingham. For this division the name "Bradfordian" is adopted.

It is admitted, however, that shelly limestones of similar character occur apparently on different horizons in the Great Oolite Series of Northampton, Rutland, and Lincoln. Here the divisions were originally made out by Prof. Judd and the late Samuel Sharp, and are as follows:—

Great Oolite Clay. Great Oolite Limestone. Upper Estuarine Series.

Amongst the fossils of the Upper Estuarine Mr. Woodward, we observe, includes *Paludina*. Without doubt this genus might very well be expected to occur in Bathonian Estuarine Beds, since *Paludina scotica* abounds in the Loch Staffin Group ("*infra* Oxfordian"). Yet there had been no previous mention of *Paludina* in Sharp's lists, so that it would be interesting to know on what authority this genus is quoted. As the Bathonian Beds are traced towards the Humber, the evidence favours the view that there is but one stratigraphical division, representing the Upper Estuarine Series, the Great Oolite Limestone, and the Great Oolite Clay.

The Cornbrash naturally terminates the series of Lower Oolitic Rocks, although the author observes that there is no palæontological break in the South of England or elsewhere between the Cornbrash and the Oxfordian Series; since in the Kellaways Rock we find, more or less abundantly, some of the characteristic fossils of the Cornbrash. As a proof of the solidarity of the Bathonian Series we would point out that *Am. discus*, figured on page 432 as a Cornbrash fossil, is also quoted from beds as low down as the Stonesfield Slate.

This serves to show that the Cornbrash is rightly regarded as a member of the Lower Oolites, whilst its palæontological connection with the Kellaways Rock is perhaps not quite so strong as the author would have us believe. The strong Cephalopod facies which accompanies the incoming of Oxfordian Beds—regarding the Kellaways Rock as the base of these—makes the palæontological distinction almost as remarkable as that indicated by the lithology. In common with the rest of the Lower Oolites, this most persistent band of calcareous rock becomes of less importance in the north of Lincolnshire and finally disappears before the Humber is reached.

The remaining chapters of the volume treat of Scenery and Agriculture, of Economic Products, and of Springs and Water Supply. Apart from the importance of the Northamptonshire iron-ore, we cannot forget that the Lower Oolites have furnished some of the most celebrated of our building-stones; whilst we must also bear in mind that the Thames is largely indebted for its waters to the peculiar configuration of these rocks in the Cotteswolds. The curious facts connected with the distribution of saline waters in the Jurassic Rocks generally have not escaped Mr. Woodward's observation; more especially noteworthy being the Forest Marble touched in the Swindon borehole, which contained water carrying 2,131 grains of saline matter per gallon.

A full catalogue of fossils from the Lower Oolitic Rocks of England completes the volume. Mr. Woodward is to be congratulated on this exhaustive work, in the preparation of which an immense amount of literature has been consulted, and the whole put together in the light of a personal experience in the field extending over several years. The volume will be invaluable to those who desire to become acquainted with the Lower Oolitic Rocks of this country. But while we consider that the author has done his duty thoroughly, it is difficult to say much in praise of the illustrations. Some of the figures of fossils are characteristic, but the sections are often indistinctly printed, whilst the reference numbers are frequently illegible. We would recommend those who are responsible for bringing out the Survey Memoirs to inspect a recent Number of the Proceedings of the Geologists' Association, where a section at Finedon Hill, in Northamptonshire (from a photograph), disposes of pretty nearly the whole of the Lower Oolites, as developed in that district, within a vertical space of 46 feet. The distinctness of the numbering in this case is a satisfactory feature which the Queen's printers would do well to imitate.

II.—On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma. By Dr. Fritz Noetling, F.G.S., Palæontologist, Geological Survey of India. With a Plate. (In The Records of the Geological Survey of India, vol. xxvii. part 3, 1894, pages 101–103.)

In the Tertiaries near Yenangyoung, where Dr. Noetling has lately been mapping the Oil-field of that district in Burma, he distinguishes three distinct groups of strata, which, in descending order, are—

3. Group C. Hard and soft sand-rocks, alternating with light-brown clays. Silicified wood common, and remains of land and fresh-water animals. Thickness not less than 4620 feet.

2. Group B. Brown and red sandstones and light-brown clays, containing numerous crystals of selenite, and locally countless numbers of *Batissa Crawfordi*, Noetl.; terminating in a bed of ferruginous conglomerate with numerous remains of terrestrial animals, among which *Hippotherium* [*Hipparion*] *antelopinum*, Cautley and Falconer, and *Rhinoceros Perimensis* preponderate. *Chipped flints locally not rare.* Thickness 1105 feet.

1. Group A. Blue clays, alternating with grey sandstones, which contain locally large quantities of petroleum. Fossils are scarce, but chiefly marine, with rolled fragments of bones and some teeth of land animals. Thickness not less than 1000 feet.

The author points out that this series exhibits a gradation from a littoral marine formation to fresh-water deposits; that the vertebrate remains belong to a fauna nearly identical with that of the Sewaliks; "that Group C (probably inclusive of Group B) must be of Upper Miocene, if not Pliocene, age"; and that the ferruginous conglomerate, containing the chipped flints, is therefore either lowest (earliest) Pliocene, or uppermost (latest) Miocene.

The well-known ossiferous Sewalik strata are regarded as Lower Pliocene by Lydekker; but Upper Miocene by Gaudry, Boyd-Dawkins, and others.

There were about a dozen specimens of worked flints found in the ferruginous conglomerate by Dr. Noetling, whose attention was drawn to them by one in particular, 45 millemètres long and 20 wide (illustrated by figs. 1, 1*a*, 1*b*), which was partly imbedded in the rock close to a fine lower molar of *Hippotherium* (fig. 6); four of the other flint specimens are shown in figs. 2, 3, 4, 5, in various aspects. These latter are more or less triangular flakes, about 25 millemètres long; and some specimens which seem to be ordinary ridge-flakes, as much as 40 mm. in length.

It is not clear why Dr. Noetling inserted a note of interrogation after the word "chipped" in the title of his paper. The specimen (fig. 1), which he terms "a rectangular flake," has been very definitely dressed; and the other smaller flakes have evidently been struck off larger flints, and probably manipulated so as to have a point and one or more sharp edges. The bettermost specimen (fig. 1) is ridged on each face, rounded at one end, truncate at the other, and has its edges almost parallel and sharp. One ridge-face retains a narrow strip of its original flat surface; and the other is sharply defined by nearly symmetrical flakings sloping up from the edges and meeting in a line along the middle. One edge of the implement is somewhat concave, either by irregular flaking, or subsequent breakage, on the face (fig. 1*a*) retaining a strip of the former plane surface.

It is apparent, therefore, that worked flints have been found by an experienced Geological Surveyor in India in a stratum of either Upper Miocene or Lower Pliocene age. In the title of his paper Dr. Noetling refers it to the "Upper Miocene."

In 1863 the Abbé L. Bourgeois, with the Abbé Delaunay and M. Bouvet, found some worked flints at the outcrop of some fresh-water beds of the lower part of the Calcaire de Beauce, above the bank of the stream of Thenay, near Pontlevoy (Loir-et-Cher), France; and in 1869, to certify their position, MM. Bourgeois and Delaunay had a pit dug on the top of the hill, at about a hundred mètres within the edge of the plateau above the valley, and believed that they discovered several well-characterized flint instruments, at a depth of six mètres, in the same marly beds of the Beauce Limestone that crop out on the hill-side below. See Bulletin Soc. Géol. France, ser. 2, vol. xx. 1863, pp. 535–542; vol. xxvi. 1869, pp. 901–902; vol. xxvii. 1870, pp. 519–520 (M. Raulin). Some geologists and archæologists have not accepted M. Bourgeois' interpretation of the facts, and others have ignored it; but there still remains the possibility of this Miocene occurrence of man-worked flints. This is strengthened by the finding of such relics in the Miocene of Burma; nor need we hesitate to allow that Man, as well as such Miocene animals as the *Hippotherium* (*Hipparion*), could, within a limited period, spread over the Asiatico-European continent, so as to constitute a real contemporaneity, and not merely a homotaxis, of the fossiliferous strata in that far-back stage of Geological History.

T. Rupert Jones.

CORRESPONDENCE.

AN ERECT TREE IN THE COAL-MEASURES.

Sir,—My brother-in-law, Mr. J. C. Brierley, of Rochdale, has sent me some photographs of a recent discovery there, which I think you might like to record in the Geological Magazine. My friend Mr. Platt, of Rochdale, may possibly describe it at greater length. It is that of a very perfect internal cast of an erect *Sigillarian* trunk rooted in the ground and rising to the unusual height of more than six feet, with a circumference at the base of about the same, and of four feet at its present summit. As now exposed there are three main roots from the trunk, each of which bifurcates. Near the base of the trunk the bark has been preserved in a carbonized form. The tree stands in a bed of Carboniferous shale in the Lower Coal-measures, which forms an insular mass in the bottom of the valley of the Roach and is topped by Boulder-clay. Apart from other interesting circumstances, the trunk is clearly *in situ*, and it affords good evidence of the very rapid accumulation under certain conditions of some of the primary rocks. It cannot be doubted that the shales which enclose it must have been deposited very rapidly, or else the trunk itself would have weathered away. It is also a proof that whatever deposited the Boulder-clay in the valley of the Roach, it could not have been an instrument of very active erosion, or such masses of soft shale as this, enclosing a perpendicular tree-trunk, would have been swept away. (See illustration on page 528.)

Henry H. Howorth.